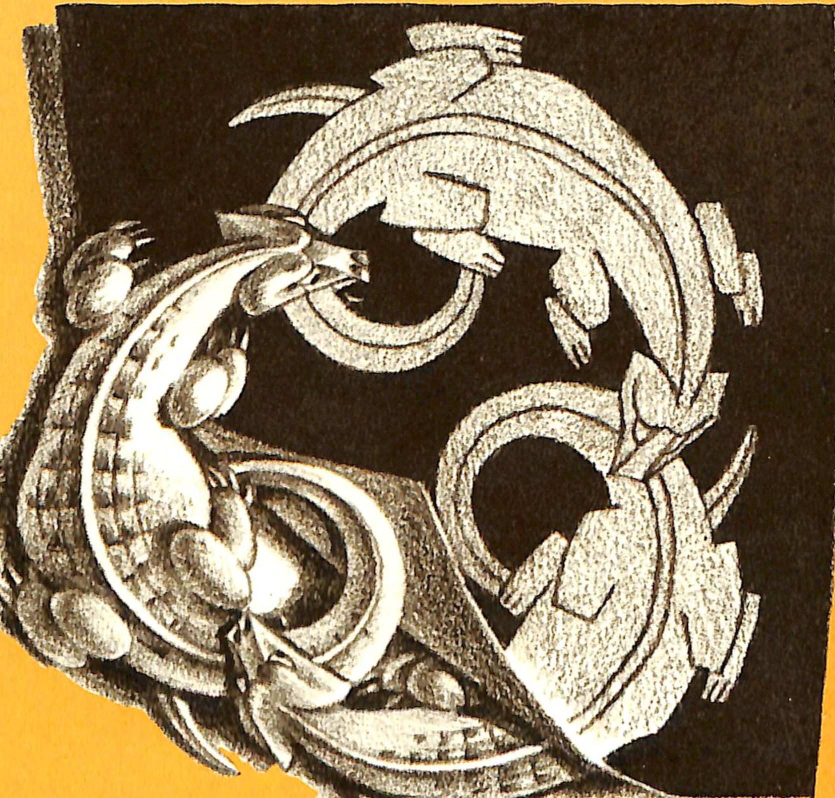
Philosophy of Science and the Occult

Patrick Grim, Editor



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Magic is not the "science" of the past. It is the science of the future. I believe that the human mind has reached a point in evolution where it is about to develop new powers—powers that would once have been considered magical. Indeed, it has always possessed greater powers than we now realize: of telepathy, premonition of danger, second sight, thaumaturgy (the power to heal); but these were part of its instinctive, animal inheritance. For the past thousand years or so, humankind has been busy developing another kind of power related to the instinct, and the result is western civilization. His unconscious powers have not atrophied; but they have "gone underground." Now the wheel has come the full circle; intellect has reached certain limits, and it cannot advance beyond them until it recovers some of its lost powers.

—Colin Wilson, from *The Occult: A History*

From Alexander's time to our own—indeed, probably for as long as human beings have inhabited this planet—people have discovered they could make money by pretending to arcane or occult knowledge. . . . Many of the impostures do not have a contemporary ring and only weakly engage our passions: it becomes clear how people in other times were deceived. But after reading many such cases, we begin to wonder what the comparable contemporary versions are. People's feelings are as strong as they always were, and scepticism is probably as unfashionable today as in any other age. Accordingly, there ought to be bamboozles galore in contemporary society. And there are. . . . The proponents of such borderline beliefs, when criticized, often point to geniuses in the past who were ridiculed. But the fact that some geniuses were laughed at does not imply that all who are laughed at are geniuses. They laughed at Columbus, they laughed at Fulton, they laughed at the Wright Brothers. But they also laughed at Bozo the Clown.

—Carl Sagan, from *Broca's Brain*

General Introduction

Philosophy of science is a paradigm of contemporary intellectual rigor. It offers a challenge of clarification, a promise of systematic understanding, and an invitation to innovative conceptual exploration. Such is its appeal.

The occult traditions are steeped in antiquity. They reach us with an atmosphere of mystery, a whisper of wisdom, and a hint of a beckoning unknown. Such is their appeal.

This is an attempt to bring the two together.

The collection that follows can be viewed in either of two ways: as an introduction to philosophy of science through an examination of the occult, or as a serious examination of the occult rigorous enough to raise central issues in philosophy of science. That the collection *can* be viewed in either of these ways is, I think, an indication of its two virtues. Its concern with issues in philosophy of science marks it as a more serious investigation of the occult than most. And its use of the occult as an introduction to philosophy of science makes it a more intriguing and more accessible introduction than most.

The editorial integration of these two is of course entirely intentional. But let me render asunder for the moment what has been so carefully brought together and address each of these aims as if it were the only aim.

I

Much that is best in current philosophy—and in philosophy for the past fifty years—falls within the loose but useful category of philosophy of science. With the benefits of increasingly sophisticated and rigorous work, however, have come unfortunate pedagogical costs. Philosophy of science is rarely seen, by students or by others as yet uninitiated into its mysteries, for the exciting ongoing exploration it truly is. Conundrums of confirmation and enigmas of explanation and puzzles of probability, as usually presented, too often fail to jar intellectual inertia or to disturb dogmatic slumbers as they

should. The collection that follows is designed to change all this; to make immediately obvious the lively excitement and intellectual fascination of philosophy of science.

For fear of misunderstanding, however, one point in particular should be emphasized. The collection is not intended to make the tedious tolerable by mere admixture with the intriguing. I think—and I think the collection demonstrates—that standard issues within the philosophy of science arise quite naturally in the context of an examination of intriguing claims concerning astrology, parapsychology, UFOs, and ancient astronauts. An examination of these areas thus offers an appealing and easily accessible forum in which to discuss theoretical issues which are of equal importance for astronomy, psychology, neutrinos, and ancient Americans. Appeal to the occult is thus mined, rather than simply mixed, for pedagogical purpose.

Topics standard to philosophy of science that arise in the following pages include issues of confirmation and selection for testing, possibility and a priori probabilities, causality and time, explanation and the nature of scientific laws, the status of theoretical entities, the problem of demarcation, theory and observation, and science and values. A sketch of where these arise in the collection accompanies the table of contents. That these issues are raised here in the context of a discussion of the occult in no way weakens the importance of dealing with them carefully and rigorously as quite general issues. But this context does serve, more effectively than most, I think, to make the initial introduction of these issues immediately understandable and dramatically compelling, and thus serves to instigate effectively the careful work they require.

In this regard the collection may serve as all that is needed within philosophy of science for a major section of an introductory humanities course, or of a course that addresses other concerns, such as issues of technology, as well. It is quite clearly suited as a core text for courses in science and pseudoscience, which have generally proven successful.[1] Within traditional philosophy of science courses it is hoped that it will serve as a useful auxiliary text, and that some of the major issues raised here will be pursued further using standard materials.

II

The collection need not be conceived, however, solely or primarily as a pedagogical tool for philosophy of science. It is also a straightforward examination of various aspects or areas of the occult.

Those areas addressed here as "the occult," for lack of a term more suitably inclusive and yet unprejudicial, are rarely met with equanimity.[2] Topics such as astrology, parapsychology, UFOs, and ancient astronauts are likely to be as threatening to the calm order of polite conversation as politics and religion were once reputed to be. Some of us have blood that boils at the insufferable

dogmatism of those who deny such areas of investigation their genuine integrity and their proper due. Others of us have blood that boils at the mere thought of the empty-headed gullibility of those impressed by such twaddle.

The theme of the collection, in most general terms, is this clash of viewpoints between impassioned defenders and icy critics. But the attempt is not to review, in tranquillity and with judicial sobriety, an intellectual battle fought elsewhere. Nor is the attempt to structure debate so as to show either the defender of the occult, or the icy skeptic, triumphant. Instead, the battle itself is allowed to rage in these pages, with equally informed and sophisticated efforts on each side of the conflict and without an editorially imposed verdict. In this regard what follows is a collection of some of the most exciting arguments offered by defenders and skeptics in conflict. There are of course various shades and various forms of both defense and critique. But with that qualification duly noted, it seems safe to number Karl Popper, Thomas S. Kuhn, Clark Glymour, and Antony Flew among the skeptics represented, with Paul Feyerabend, Michel Gauquelin, Bob Brier, and J. B. Rhine among their opponents.

In the most general terms, then, this collection is a general clash between sophisticated defenders and critics concerning topics grouped here as "the occult." But as a glimpse at the table of contents will indicate, things quickly become more complex. In anything *but* the most general terms, what is at issue is not a simple stand-off but an intricate web of challenge and response, evidence offered and disputed, argument and counter-argument. This is, of course, what is to be expected of any rigorous and systematic debate concerning these topics. Astrology, parapsychology, UFOs, and ancient astronauts raise significantly different issues and call for quite different handling, whether in critique or defense. Within each topic, moreover, there are alternative lines of defense and critique and counter-critique, different types of appeal to different types of evidence, and various ways of applying various general principles at issue. To the impatient, anxious for a glib vindication or refutation, the labyrinthine twists of complex argumentation may prove annoying. But it is precisely the intricacy and subtlety of such argument that marks a genuinely *serious* consideration of these topics, and it is a serious consideration that is intended here. It is also within the labyrinths of such careful argument that the major issues in philosophy of science, alluded to above, so clearly arise.

Let me add here a warning both to those who think of themselves as true believers and to those who think of themselves as icy skeptics with regard to the occult: one ought not expect to emerge from the philosophical discussion of the following pages, or from any serious philosophical discussion, entirely unscathed. Icy skeptics may find evidence of which they were unaware, and encounter new and unsettlingly plausible arguments against their initial stance. True believers may find that certain bodies of evidence, when actually

examined, are less compelling than they are popularly reputed to be, and may discover new and strikingly sophisticated arguments on the other side. It is fine to enter the discussion as either a believer or a skeptic, as long as one is willing to take serious debate seriously, and as long as one recognizes that that offers no guarantee that one will end up with precisely the same convictions one began with.

It should also be noted that the major philosophical problems raised in this book are problems posed for either the skeptic or the believer. These are quite general and pervasive quandaries, and no particular position with regard to the occult will allow one to wave them aside.

Here the problem of demarcation, a major theme of the collection, may serve as a convenient example. Consider first the position of the skeptic. He who rejects the occult as mere "pseudoscience" must draw a line between proper science and that which he rejects, and the problem of demarcation is precisely the problem of precisely where, and on what justifiable basis, such a line is to be drawn. Nor can the skeptic rest content with a line of demarcation placed conveniently at the edge of undebatable contemporary scientific knowledge. Science always calls for *further* investigation, and thus one must have some grounds for distinguishing some proposed lines of investigation as properly scientific, even before they are actually pursued, from other endeavors that would be merely "pseudoscientific." For the skeptic the problem of demarcation is to draw a justifiable distinction in terms general enough that it may govern decisions regarding future work as well as judgments regarding current efforts.

But consider also the case of the true believer. No matter how sympathetic one is to the various topics included as "the occult," one simply cannot, within the bounds of consistency, believe it all. The various aspects of the occult do not form a harmonious whole, and one is subjected to conflicting theories and rival explanations and contradictory claims and competing modes of investigation within occult lore just as elsewhere. Thus even the truest of believers must distinguish good work from bad, and worthy from hopeless approaches or forms of investigation. The believer, too, is forced to draw a line and to justify its placement, though his line may lie farther from recognized science and deeper within the occult. The general problem of where to draw such a line, and why to draw it there—the general problem of demarcation—is the same for skeptic and believer. Much the same holds for general issues of causality, confirmation, probability, explanation, theory and observation, and science and values which form the major conceptual currents of the volume.

III

The two aims of the collection—as an introduction to philosophy of science through an examination of the occult, and as an exciting debate concerning

the occult serious enough to broach general issues in philosophy of science—are not, I think, in competition. To a large extent, the satisfactory pursuit of either aim quite naturally furthers the other aim as well. The more serious and sophisticated that debate becomes concerning astrology, parapsychology, and the like, the more clearly philosophical it becomes as well. And the more such a discussion relies on traditional work within philosophy of science, the more sophisticated it is bound to be.

The table of contents is accompanied by a chart indicating where in the collection standard issues in the philosophy of science arise. Some more complete comments regarding the structure of the volume, however, may also be of help.

Each of the major sections of the collection opens with a brief introductory overview, designed to raise the particular issues of that section, to stress points of continuity with other sections, and to emphasize crucial patterns of argument that develop in the course of that section. The last of these is perhaps the most important. Within each section pieces have been chosen and placed so as to offer a continuous thread of developing argument, critique, and response through the section. Most pieces within a section offer implicit, and sometimes explicit, replies to the pieces that precede them or further developments of earlier arguments. The first selections within each section are generally the simplest and most immediately accessible, and are followed by pieces of increasing sophistication as the argument develops. So although pieces may be selected from the whole for individual attention, there is also a continuity within each section that recommends that the pieces it includes be read in the order in which they appear. It is this thread of continuity that I attempt to make more explicit in introductory outlines.

In some ways the section that is theoretically most fundamental for the collection as a whole is not the first section but the second, which offers a fairly thorough discussion of the issue of demarcation in general. The most common rejection of astrology, parapsychology, UFO studies, and the like is to brand them with the epithet "pseudoscience." But what is it that lies behind that epithet; what does it mean to call something either "scientific" or "pseudoscientific," and what is it in general that will justify the praise of an endeavor as "scientific" or will vindicate its vilification as "pseudoscientific"? This is Popper's "problem of demarcation," and if the collection as a whole addresses one central issue, it is the problem of demarcation. It must be added immediately, however, that it is far from clear that the collection *does* address any one central issue alone. Issues in confirmation theory, questions concerning explanation and scientific laws, theory and observation, experimental design, causality, and probability all come in for their share of attention. It may be, moreover, that the "problem of demarcation" is not itself, in the end, a single issue, but instead a complex tangle of different considerations in which each of these other questions has a place. This is to

anticipate the discussion of the second section, however, whereas my purpose here is merely to introduce it as a major theoretical component of the collection. In this section both Karl Popper and Thomas S. Kuhn are represented, with comments both critical and elucidative by Daniel Rothbart and Robert Feleppa. Clark Glymour and Douglas Stalker offer a humorous lampoon of alleged pseudoscience to open the section, and Roger Cooter offers the view of a historian critical of the whole philosophical attempt at demarcation.

The first section of the collection is devoted to astrology. Its appearance before the more general theoretical discussion of demarcation is as I would recommend that it be read and as I would use it in my own teaching. The intricacies of complex argument concerning demarcation can seem twisted indeed, especially to the philosophically uninitiated, without a particular problem and a specific case to keep in mind in pursuing them. Astrology is offered first as such a specific case, with its legitimacy or illegitimacy a problem the more complete examination of which calls for further work in the later section.

The section on astrology opens with a critical broadside, excoriating astrology in no uncertain terms and signed by 186 scientific notables. That attack is immediately answered, however, by Paul Feyerabend in the role of critics' critic. This section also contains an impressive report of statistical work by Michel Gauquelin, work that has quite widely been applauded as vindicating astrology. Edward W. James gives a disparaging glance at the popular literature of astrology, alluding to the work of Popper and Kuhn that appears later in the collection, and I. W. Kelly offers a fairly comprehensive review of various types of astrology and attempts to gauge dispassionately their strengths and weaknesses. This first section does not impose, and is not meant to impose, any clear resolution concerning the legitimacy of astrology. Thus it is no weakness if some readers find the work of Gauquelin and its review by Kelly so impressive as to establish at least a core of legitimacy for astrology, whereas other readers find compelling the critical comments in the opening piece, or in James's or Kelly's pieces. The section is intended instead to evoke, in the context of lively debate concerning astrology, some broader philosophical issues that call for further pursuit. The general issue of demarcation is primary among these, and thus leads directly into the succeeding section. But Gauquelin's piece also offers an opportunity for further thought regarding causality and correlation, and Feyerabend and James introduce a number of informal fallacies worthy of consideration. The difficulties of deciding what does or would serve as evidence for astrology may offer quite general lessons regarding hypothesis formulation and testing.

Using the initial consideration of astrology and the further development of important conceptual tools in the general discussion of demarcation as a background, the next section—on parapsychology—renews some earlier

questions and poses important new ones. Here, to begin with, is another specific case against which to judge proposals for demarcation outlined in the previous section. Is *parapsychology* to be classified as mere pseudoscience, and if so why? The readings in this section indicate major ways in which the case of parapsychology differs from that of astrology, and thus emphasize the many different considerations that may be at issue in any serious attempt to draw a justifiable line between science and its pretenders. But parapsychology also raises important philosophical issues not previously addressed in the collection. What does it mean to wave something aside as "mere coincidence," and on what basis do we distinguish phenomena that call for an explanation in their own right from phenomena that do not? What is the proper role in science of intuitive principles such as our conviction that mental phenomena must in some way involve brains or that all knowledge must ultimately be acquired by some sensory means? Is it possible for an effect to precede its cause?

The parapsychology section opens with an ambitious review of major parapsychological research to date, and a discussion of promising current developments in the field, by Ruth Reinsel. This offers a helpful summary of documented work—rather than merely anecdotal cases or rumored tests that abound as popular lore—against which to gauge the more general and philosophical treatments that follow. The last piece of the section is by J. B. Rhine, by far the most noteworthy and respected figure in the development of the field, and offers a direct response to accusations of fraud as well as outlining the recently discovered experimental falsifications of Walter J. Levy, Jr. Noteworthy among the critics in this section is Antony Flew. His contribution touches on Hume's comments regarding miracles and would easily serve as an introduction to questions concerning lawlikeness and the status of scientific laws. Other issues broached in the critical pieces by Flew, Galen K. Pletcher, and Jane Duran include repeatability, units of explanation, the scientific status of a priori principles, theoretical entities, and causality and correlation. Duran's piece is also noteworthy for raising central questions in the analysis of knowledge and the issue of identity theory in philosophy of mind.

An issue dealt with quite thoroughly in the parapsychology section is that of "backward causation"; can an effect precede its cause? The claim that this is not possible, and thus that precognition is not possible, is first introduced by Galen Pletcher and is more fully developed as a strong critical point in the pieces by Flew and Duran. Their skeptical conclusions are quite forecefully challenged, however, by Bob Brier and Maithili Schmidt-Raghavan. Brier and Schmidt-Raghavan suggest that this standard attack rests on a confusion, and attempt to vindicate parapsychology (and their own parapsychological experiment) by arguing that backward causation is indeed possible. Here Brier and Schmidt-Raghavan rely in part on the work of John Stuart Mill.

The parapsychology section dramatically broadens the number of issues within philosophy of science at stake in the evaluation of various aspects of the occult. The following section on UFOs and ancient astronauts broadens this set of issues still further. This section opens with a selection of classic UFO cases conceded to be unexplained in the Condon Report, drafted under the auspices of the University of Colorado under contract with the United States Air Force. J. Allen Hynek appears among the investigators. This opening piece is immediately followed by a "debunking" effort directed against some typical cases by Philip J. Klass. Side by side these pieces raise, albeit implicitly, difficulties regarding theory-laden description of evidence and the reliability of observation. Genuinely philosophical themes are more explicitly developed in T. Patrick Rardin's contribution. Rardin initially discusses some important conceptual difficulties in the definition of "UFOs" that inevitably infect the formulation of hypotheses concerning them. Most importantly, however, he raises questions concerning a priori probability assignments with regard to extraterrestrial hypotheses in a systematic way, both criticizing a probabilistic argument offered by Carl Sagan and attempting to improve on it using an appeal to rational decision procedures.

Also included in this section is a vituperative exchange regarding ancient astronauts between Pasqual S. Schievella, a philosophical defender of the work of Erich von Däniken, and Mary Vetterling-Braggin, a philosophical critic of von Däniken. The debate here turns on distinctions between logical possibility and grades of epistemic possibility, again raises issues regarding theory-laden description, and effectively introduces questions regarding hypothesis selection for testing.

The final section of the collection, regarding other approaches to the occult, differs importantly in kind from its predecessors. As noted above, "the occult" is used throughout the volume as a general term for topics such as astrology, parapsychology, UFOs, and ancient astronauts, and is used in this capacity simply for lack of a better term. The final section addresses the possibility that this mode of procedure, and the general tone of examination given these topics in the earlier sections, may have unfairly distorted or disparaged other fully legitimate human endeavors properly included in the occult. Is a mistake being made in judging some things against a scientific standard or with the model of science in mind? William James's classic remarks on mysticism and nitrous oxide are used to open the section, and James proposes an affirmative answer to this question. There are perhaps, James suggests, different orders of experience beyond the reach of science, and these may provide the "truest of insights into the meaning of this life." Such a position is further argued by Edward Conze, who quite explicitly accuses western "science-bound philosophers" of perversely refusing to accept other traditions as on an equal footing with science as we know it. Such a position will be familiar to readers of Carlos Castaneda and Alan Watts,

among others, but Conze's work offers a more straightforward and in many ways more compelling argument. What James and Conze raise is the specter of epistemological relativism, and one of the things I attempt to do in the final piece is to offer a partial reply. I attempt to do so, however, by addressing the question of whether science is value-laden, and what this might mean in considering science as but one among rival alternatives. That my piece comes last is of course by no means to suggest that mine is to be taken as anything like the final word on the matter. This section as a whole is designed to broach issues regarding science and values that do not emerge so clearly elsewhere in the collection. But it is also intended to offer a context in which issues of epistemological relativism, so often introduced by students, can be clearly and profitably discussed. Thus it may prove pedagogically useful to skip to the final section when the issue arises.

At the end of each major section is appended a brief list of suggested readings. These are by no means intended as comprehensive bibliographies of either the topic of the section or its philosophical treatment. They are intended instead as non-intimidating lists of easily available sources that may be of use as a next step in the student's research or in selecting additional readings within a course.

The goals of the collection as a whole, then, are to provide an appealing introduction to philosophy of science by means of a serious examination of aspects or areas of the occult, and to offer an examination of the occult serious enough to grapple with central issues in philosophy of science. The second goal, I think, is well met. The goal of presenting an introduction to philosophy of science as exciting and engaging as the subject deserves still calls for creative and responsive teaching, which no course materials can themselves provide, but for which I hope this collection will be a useful aid.

IV

Some of the pieces included here are reprinted from other sources. I am grateful to Paul Kurtz and the editors of *The Humanist* for permission to reprint "Objections to Astrology"; to Paul Feyerabend for "The Strange Case of Astrology" and to Michel Gauquelin for "Spheres of Influence"; to Sir Karl Popper for permission to reprint from "Science: Conjectures and Refutations"; to Thomas S. Kuhn and Cambridge University Press for permission to reprint from "Logic of Discovery or Psychology of Research?"; to Roger Cooter, Wilfrid Laurier University Press, and the editors of *Science, Pseudoscience, and Society* for permissions regarding "The Conservatism of 'Pseudoscience' "; to Antony Flew, the *Pacific Philosophical Quarterly*, Wilfrid Laurier University Press, and the editors of *Science, Pseudoscience, and Society* for permission to reprint "Parapsychology: Science or Pseudo-Science?"; to Louisa E. Rhine and the editors of the *Journal of Parapsychology* for permission to reprint from "Second Report on a Case of Experi-

menter Fraud"; to the University of Colorado Press for permission to reprint from the Condon Report; to Philip J. Klass and the editors of *The Humanist* for permission to reprint "UFOs: Fact or Fantasy?"; and to Professor Ninian Smart, Muriel Conze, and George Allen and Unwin Ltd. for permission to reprint from "Tacit Assumptions." The opening quotations from Colin Wilson, *The Occult: A History* (New York: Random House, 1971), p. 39, and Carl Sagan, *Broca's Brain* (New York: Random House, 1979), pp. 3 and 18, are with permission. I am also indebted to the contributors for tolerating so many requests for laborious revision in the name of coherency and effectiveness within the collection.

Notes

1. See also Michael Martin, "The Use of Pseudo-Science in Science Education," *Science Education* 55 (1971): 53–56; and Paul Thagard and Daniel M. Hausman, "Sun Signs vs. Science: Using Astrology to Teach Philosophy of Science," *Metaphilosophy* 11 (1980): 101–104.

2. It is intriguing that a suitably broad but unprejudicial term to include astrology, parapsychology, UFOs and ancient astronauts, and the like is so difficult to find. "Pseudoscience," of course, condemns as it classifies. "The paranormal" seems more properly confined to parapsychology, and "the occult" seems more properly to include astrology and witchcraft than UFOs. Recourse to metaphor does little better. "The frontiers of science" seems too laudatory, as do "the edges of science" or "the borders of science," though less so. "The fringes of science," on the other hand, has precisely the opposite drawback. "Borderlands of science" is perhaps better, though its ambiguity extends not merely to normative overtones but leaves one wondering precisely what topics are at issue.

SECTION I

ASTROLOGY

Introduction

In the next section we will consider a number of rival attempts to distinguish science from pseudoscience in general. Here, however, we will start by considering one particular controversial area in some detail: the case of astrology.

Is astrology a pseudoscience? As we will see later, many philosophers of science take astrology to be a paradigm or a prime example of pseudoscience. On this particular case they appear to be unanimous, however much they may disagree as to precisely *why* astrology is pseudoscientific or as to precisely what astrology's faults are.

Our first selection here presents a similarly unanimous condemnation of astrology. One hundred eighty-six prominent scientists, including eighteen Nobel Prize winners, have signed a statement blasting "the pretentious claims of astrological charlatans." The claims of astrology, they hold, have no scientific basis and are moreover claims against which we have strong evidence. The prominent place of astrology in the media "can only contribute to the growth of irrationalism and obscurantism."

In the piece immediately following, however, PAUL FEYERABEND attacks this statement for its religious tone, its blatant appeal to authority, and its lack of adequate argument. Those who have signed the statement, Feyerabend claims, simply "do not know what they are talking about," and he presents a number of scientific results that appear to contradict the general claims of the statement. Feyerabend is careful to note that he is not attempting to defend the practice of most contemporary astrologers. But it is clear that he thinks astrology cannot fairly be confronted, and that science in general is very poorly represented, by attacks of this kind.

In "On Dismissing Astrology and Other Irrationalities," EDWARD W. JAMES sharply criticizes attempts to make short work of astrology by quickly dismissing it as irrational. James briefly outlines Karl Popper's and

Thomas S. Kuhn's attempts to dismiss pseudoscience, more fully presented in the following section, and argues that neither of these attempts is successful in the case of astrology. Although a handful of other common arguments may give us good reason to disbelieve the claims of astrology, James further suggests, these arguments fail as sufficient grounds for dismissing astrology and those who uphold it as irrational. In the end James holds that we *do* know that astrology is irrational, not on the basis of any simple criterion for distinguishing between science and pseudoscience and not on the basis of any small group of telling arguments, but because we can see that astrological literature is a complex tangle of fallacy and misrepresentation. Thus any serious attempt to show the irrationality of astrology, James concludes, must take the tedious and unpleasant route of critically examining astrological literature step by step and argument by argument.

James mentions in passing the work of MICHEL GAUQUELIN, who speaks for himself in the following selection. Gauquelin does not consider himself an astrologer, and explicitly rejects zodiacal influences and the notion that external signs in the stars and mysterious astrological connections control our fates so as to "make puppets out of us." But on the basis of over twenty years of statistical research Gauquelin does claim to have shown a link between planetary movements and human character, and thus to have revealed a "golden grain of truth in astrological superstition." In the selection presented here, Gauquelin offers some of his most startling results, which seem to indicate that there is a statistically significant correlation between success in certain careers and the position of certain planets at one's birth. Gauquelin also outlines his methodology in some detail and briefly suggests an explanation for his findings. Here the reader should consider once again James's earlier critical comments in light of this more complete outline of Gauquelin's research.

Gauquelin's work is further discussed in the second section of I. W. KELLY'S concluding piece. But Kelly also offers a critical examination of traditional astrology and of Dane Rudhyar's "Humanistic astrology." Traditional astrology Kelly rejects on the basis of an impressive survey of relevant research, and "Humanistic astrology" on the basis of more purely philosophical doubts. Of the three, Gauquelin's "cosmobiology" clearly comes out the best in Kelly's treatment; he concedes that Gauquelin and his collaborators "have amassed a large amount of evidence that prima facie supports his hypothesis." But Kelly also calls for further research before any firm conclusions can be drawn.

Throughout this section the reader should attempt to tackle the question of the status of astrology, scientific or otherwise. *Is* astrology a prime example of pseudoscience, and if so why? Must we distinguish traditional astrology from "Humanistic astrology" and "cosmobiology" in order to answer such a question? With this background examination of the case of astrology in

particular, the general question of what distinguishes science from pseudoscience in general will lead us into the next section.

The selections of this section also raise some other important questions. What is the relation between correlation, such as it appears in Gauquelin's data, and claims regarding causal relations between those things that are correlated? What precisely *is* the theory or hypothesis of astrology, and how might it be effectively tested? Is the attitude of "Objections to Astrology" a justifiable one? Is Gauquelin's work correctly taken as a vindication of astrology?

Objections to Astrology

A Statement by 186 Leading Scientists

Scientists in a variety of fields have become concerned about the increased acceptance of astrology in many parts of the world. We, the undersigned—astronomers, astrophysicists, and scientists in other fields—wish to caution the public against the unquestioning acceptance of the predictions and advice given privately and publicly by astrologers. Those who wish to believe in astrology should realize that there is no scientific foundation for its tenets.

In ancient times people believed in the predictions and advice of astrologers because astrology was part and parcel of their magical world view. They looked upon celestial objects as abodes or omens of the Gods and, thus, intimately connected with events here on earth; they had no concept of the vast distances from the earth to the planets and stars. Now that these distances can and have been calculated, we can see how infinitesimally small are the gravitational and other effects produced by the distant planets and the far more distant stars. It is simply a mistake to imagine that the forces exerted by stars and planets at the moment of birth can in any way shape our futures. Neither is it true that the position of distant heavenly bodies make certain days or periods more favorable to particular kinds of action, or that the sign under which one was born determines one's compatibility or incompatibility with other people.

Why do people believe in astrology? In these uncertain times many long for the comfort of having guidance in making decisions. They would like to believe in a destiny predetermined by astral forces beyond their control. However, we must all face the world, and we must realize that our futures lie in ourselves, and not in the stars.

One would imagine, in this day of widespread enlightenment and edu-

cation, that it would be unnecessary to debunk beliefs based on magic and superstition. Yet, acceptance of astrology pervades modern society. We are especially disturbed by the continued uncritical dissemination of astrological charts, forecasts, and horoscopes by the media and by otherwise reputable newspapers, magazines, and book publishers. This can only contribute to the growth of irrationalism and obscurantism. We believe that the time has come to challenge directly, and forcefully, the pretentious claims of astrological charlatans.

It should be apparent that those individuals who continue to have faith in astrology do so in spite of the fact that there is no verified scientific basis for their beliefs, and indeed that there is strong evidence to the contrary.

Bart J. Bok, *emeritus professor of astronomy University of Arizona*

Lawrence E. Jerome *science writer Santa Clara, California*

Paul Kurtz *professor of philosophy SUNY at Buffalo*

NOBEL PRIZEWINNERS

Hans A. Bethe, *professor emeritus of physics, Cornell*
Sir Francis Crick, *Medical Research Council, Cambridge, England*
Sir John Eccles, *distinguished professor of physiology and biophysics, SUNY at Buffalo*
Gerhard Herzberg, *distinguished research scientist, National Research Council of Canada*
Wassily Leontief, *professor of economics, Harvard University*
Konrad Lorenz, *univ. prof., Austrian Academy of Sciences*
André M. Lwoff, *honorary professor, Institut Pasteur, Paris*
Sir Peter Medawar, *Medical Research Council, Middlesex, Eng.*
Robert S. Mulliken, *dist. prof. of chemistry, U. of Chicago*
Linus C. Pauling, *professor of chemistry, Stanford University*
Edward M. Purcell, *Gerhard Gade univ. prof. Harvard Univ.*
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N. Tinbergen, *emer. professor of animal behavior, Oxford Univ.*
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The Strange Case of Astrology

PAUL FEYERABEND

To drive the point home I shall briefly discuss the "Statement of 186 Leading Scientists" against astrology which appeared in the September/October issue 1975 of the *Humanist*. This statement consists of four parts. First, there is the statement proper which takes about one page. Next come 186 signatures by astronomers, physicists, mathematicians, philosophers and individuals with unspecified professions, eighteen Nobel Prize Winners among them. Then we have two articles explaining the case against astrology in detail.

Now what surprises the reader whose image of science has been formed by the customary eulogies which emphasize rationality, objectivity, impartiality and so on is the religious tone of the document, the illiteracy of the "arguments" and the authoritarian manner in which the arguments are being presented. The learned gentlemen have strong convictions, they use their authority to spread these convictions (why 186 signatures if one has arguments?), they know a few phrases which sound like arguments, but they certainly do not know what they are talking about.[1]

Take the first sentence of the "Statement." It reads: "Scientists in a variety of fields have become concerned about the increased acceptance of astrology in many parts of the world."

In 1484 the Roman Catholic Church published the *Malleus Maleficarum*, the outstanding textbook on witchcraft. The *Malleus* is a very interesting book. It has four parts: phenomena, aetiology, legal aspects, theological aspects of witchcraft. The description of phenomena is sufficiently detailed to enable us to identify the mental disturbances that accompanied some cases. The aetiology is pluralistic, there is not just the official explanation, there are other explanations as well, purely materialistic explanations included. Of

course, in the end only one of the offered explanations is accepted, but the alternatives are discussed and so one can judge the arguments that lead to their elimination. This feature makes the *Malleus* superior to almost every physics, biology, chemistry textbook of today. Even the theology is pluralistic, heretical views are not passed over in silence, nor are they ridiculed; they are described, examined, and removed by argument. The authors know the subject, they know their opponents, they give a correct account of the positions of their opponents, they argue against these positions and they use the best knowledge available at the time in their arguments.

The book has an introduction, a bull by Pope Innocent VIII, issued in 1484. The bull reads: "It has indeed come to our ears, not without afflicting us with bitter sorrow, that in . . ."—and now comes a long list of countries and counties—"many persons of both sexes, unmindful of their own salvation have strayed from the Catholic Faith and have abandoned themselves to devils . . ." and so on. The words are almost the same as the words in the beginning of the "Statement," and so are the sentiments expressed. Both the Pope and the "186 leading scientists" deplore the increasing popularity of what they think are disreputable views. But what a difference in literacy and scholarship!

Comparing the *Malleus* with accounts of contemporary knowledge the reader can easily verify that the Pope and his learned authors knew what they were talking about. This cannot be said of our scientists. They neither know the subject they attack, astrology, nor those parts of their own science that undermine their attack.

Thus Professor Bok, in the first article that is attached to the statement writes as follows: "All I can do is state clearly and unequivocally that modern concepts of astronomy and space physics give no support—better said, negative support—to the tenets of astrology" i.e. to the assumption that celestial events such as the positions of the planets, of the moon, of the sun influence human affairs. Now, "modern concepts of astronomy and space physics" include large planetary plasmas and a solar atmosphere that extends far beyond the earth into space. The plasmas interact with the sun and with each other. The interaction leads to a dependence of solar activity on the relative positions of the planets. Watching the planets one can predict certain features of solar activity with great precision. Solar activity influences the quality of short wave radio signals hence fluctuations in this quality can be predicted from the position of the planets as well.[2]

Solar activity has a profound influence on life. This was known for a long time. What was not known was how delicate this influence really is. Variations in the electric potential of trees depend not only on the *gross* activity of the sun but on *individual flares* and therefore again on the positions of the planets.[3] Piccardi, in a series of investigations that covered more than thirty years found variations in the rate of standardized chemical reactions

that could not be explained by laboratory or meteorological conditions. He and other workers in the field are inclined to believe "that the phenomena observed are primarily related to changes of the structure of water used in the experiments."[4] The chemical bond in water is about one tenth of the strength of average chemical bonds so that water is "sensitive to extremely delicate influences and is capable of adapting itself to the most varying circumstances to a degree attained by no other liquid."[5] It is quite possible that solar flares have to be included among these "varying circumstances"[6] which would again lead to a dependence on planetary positions. Considering the role which water and organic colloids[7] play in life we may conjecture that "it is by means of water and the aqueous system that the external forces are able to react on living organisms."[8]

Just how sensitive organisms are has been shown in a series of papers by F. R. Brown. Oysters open and close their shells in accordance with the tides. They continue their activity when brought inland, in a dark container. Eventually they adapt their rhythm to the new location which means that they sense the very weak tides in an inland laboratory tank.[9] Brown also studied the metabolism of tubers and found a lunar period though the potatoes were kept at constant temperature, pressure, humidity, illumination: man's ability to keep conditions constant is smaller than the ability of a potato to pick up lunar rhythms[10] and Professor Bok's assertion that "the walls of the delivery room shield us effectively from many known radiations" turns out to be just another case of a firm conviction based on ignorance.

The "Statement" makes much of the fact that "astrology was part and parcel of (the) magical world view" and the second article that is attached to it offers a "final disproof" by showing that "astrology arose from magic." Where did the learned gentlemen get *this* information? As far as one can see there is not a single anthropologist among them and I am rather doubtful whether anyone is familiar with the more recent results of this discipline. What they do know are some *older* views from what one might call the "Ptolemaic" period of anthropology when post-17th century Western man was supposed to be the sole possessor of sound knowledge, when field studies, archaeology and a more detailed examination of myth had not yet led to the discovery of the surprising knowledge possessed by ancient man as well as by modern "Primitives" and when it was assumed that history consisted in a simple progression from more primitive to less primitive views. We see: the judgement of the "186 leading scientists" rests on an antediluvian anthropology, on ignorance of more recent results in their own fields (astronomy, biology, and the connection between the two) as well as on a failure to perceive the implications of results they do know. It shows the extent to which scientists are prepared to assert their authority even in areas in which they have no knowledge whatsoever.

There are many minor mistakes. "Astrology," it is said "was dealt a serious death blow" when Copernicus replaced the Ptolemaic system. Note the wonderful language: does the learned writer believe in the existence of "death blows" that are not "serious"? And as regards the content we can only say that the very opposite was true. Kepler, one of the foremost Copernicans used the new discoveries to improve astrology, he found new evidence for it, and he defended it against opponents. [11] There is a criticism of the dictum that the stars incline, but do not compel. The criticism overlooks that modern hereditary theory (for example) works with inclinations throughout. Some specific assertions that are part of astrology are criticized by quoting evidence that contradicts them; but every moderately interesting theory is always in conflict with numerous experimental results. Here astrology is similar to highly respected scientific research programmes. There is a longish quotation from a statement by psychologists. It says: "Psychologists find no evidence that astrology is of any value whatsoever as an indicator of past, present, or future trends of one's personal life. . . ." Considering that astronomers and biologists have not found evidence *that is already published, and by researchers in their own fields*, this can hardly count as an argument. "By offering the public the horoscope as a substitute for honest and sustained thinking, astrologers have been guilty of playing upon the human tendency to take easy rather than difficult paths"—but what about psychoanalysis, what about the reliance upon psychological tests which long ago have become a substitute for "honest and sustained thinking" in the evaluation of people of all ages? [12] And as regards the magical origin of astrology one need only remark that science once was very closely connected with magic and must be rejected if astrology must be rejected on these gounds.

The remarks should not be interpreted as an attempt to defend astrology *as it is practiced now* by the great majority of astrologists. Modern astrology is in many respects similar to early mediaeval astronomy: it inherited interesting and profound ideas, but it distorted them, and replaced them by caricatures more adapted to the limited understanding of its practitioners. [13] The caricatures are not used for research; there is no attempt to proceed into new domains and to enlarge our knowledge of extra-terrestrial influences; they simply serve as a reservoir of naive rules and phrases suited to impress the ignorant. Yet this is not the objection that is raised by our scientists. They do not criticize the air of stagnation that has been permitted to obscure the basic assumptions of astrology, they criticize these basic assumptions themselves and in the process turn their own subjects into caricatures. It is interesting to see how closely both parties approach each other in ignorance, conceit and the wish for easy power over minds. [14]

Notes

1. This is quite literally true. When a representative of the BBC wanted to interview some of the Nobel Prize Winners they declined with the remark that they had never studied astrology and had no idea of its details. Which did not prevent them from cursing it in public. In the case of Velikowski the situation was exactly the same. Many of the scientists who tried to prevent the publication of Velikowski's first book or who wrote against it once it had been published never read a page of it but relied on gossip or on newspaper accounts. This is a matter of record. Cf. de Grazia, *The Velikowski Affair*, New York 1966, as well as the essays in *Velikovsky Reconsidered*, New York 1976. As usual the greatest assurance goes hand in hand with the greatest ignorance.

2. J. H. Nelson, *RCA Review*, Vol. 12 (1951), pp. 26ff.; *Electrical Engineering*, Vol. 71 (1952), pp. 421ff. Many of the scientific studies that are relevant for our case are described and indexed in Lyall Watson, *Supernature*, London 1973. Most of these studies have been neglected (without criticism) by orthodox scientific opinion.

3. This was found by H. S. Burr. Reference in Watson, *op. cit.*

4. W. W. Tromp, "Possible Effects of Extra-Terrestrial Stimuli on Colloidal Systems and Living Organisms," *Proc. 5th Intern. Biometeorolog. Congress, Nordwijk 1972*, Tromp and Bouma (eds.), p. 243. The article contains a survey of the work initiated by Piccardi who started long range studies on the causes of certain non-reproducible physico-chemical processes in water. Some of the causes were related to solar eruptions, others to lunar parameters. Reference to such extra-terrestrial stimuli is rare among environmental scientists and the corresponding problems are "often forgotten or neglected" (p. 239). However, "despite a certain resistance experienced among orthodox scientists, a clear breakthrough can be observed in recent years amongst the younger research workers" (p. 245). There are special research centres such as the *Biometeorological Research Center* in Leiden and the *Stanford Research Center* in Menlo Park, California which study what once was called the influence of the heavens upon the earth and have found correlations between organic and unorganic processes and lunar, solar, planetary parameters. Tromp's article contains a survey and a large bibliography. The *Biometeorological Research Center* issues periodic lists of publications (monographs, reports, publications in scientific journals). Part of the work done at the *Stanford Research Institute* and related institutions is reported in (ed.) John Mitchell *Psychic Exploration, A Challenge for Science*, New York 1974.

5. G. Piccardi, *The Chemical Basis of Medical Climatology*, Springfield, Illinois 1962.

6. Cf. G. R. M. Verfaillie, *Intern. Journ. Biometeorol.*, Vol. 13 (1969), pp. 113ff.

7. Tromp, *loc. cit.*

8. Piccardi, *loc. cit.*

9. *Am. Journ. Physiol.*, Vol. 178 (1954), pp. 510ff.

10. *Biol. Bull.* Vol. 112 (1957), p. 285. The effect could also be due to synchronicity–cf. C. G. Jung, "Synchronicity: An Acausal Connecting Principle," in *The Collected Works of C. G. Jung*, Vol. 8, London 1960, pp. 419ff.

11. Cf. Norbert Herz, *Keplers Astrologie*, Vienna 1895, as well as the relevant passages from Kepler's collected works. Kepler objects to tropical astrology, retains sidereal astrology, but only for mass phenomena such as wars, plagues etc.

12. The objection from free will is not new; it was raised by the Church fathers. So was the twin objection.

13. On astrology see Paul Feyerabend, *Against Method*, p. 100n.

14. Cf. *Against Method* p. 208n.

On Dismissing Astrology and Other Irrationalities

Edward W. James

How do we know that the irrational—say, astrology—is so? Or is this question peccantly complex, assuming before showing that we do know? To answer these questions we must consider the received views, which seek to dismiss astrology and its kin by appealing either to methodology or content. Consider first, then, two of our leading methodologists—Sir Karl Popper and Thomas S. Kuhn.

Popper claims that astrology is to be rejected because it fails to seek out falsifications, a task that for Popper marks the rational person. He tells us: "Astrologers were greatly impressed, and misled, by what they believed to be confirming evidence—so much so that they were quite unimpressed by any unfavorable evidence."[1] Alluring as this observation may be, however, it fails to close the door on astrology. For if Popper means that astrology, past or present, neither makes specific predictions nor seeks to revise its theories when the predictions fail, then he errs factually. For astrologers repeatedly predict on the basis of their theories and revise on the basis of their failures. We see this, for instance, in the astrological theory of subsumption. If a person whose horoscope predicts fortune instead encounters failure, the astrologer seeks to explain this by subsuming the relevant horoscope under a more inclusive one that calls for misfortune. Hence it was the horoscope of Hiroshima that proved dominant during the waning days of World War II. Yet if Popper means that astrology fails to put itself *as a whole* to the test, then he errs conceptually. For astrology claims to be a discipline or area of study, not a specific theory or law, and hence is no more to be falsified than physics or astronomy.

Let us then turn to Thomas S. Kuhn's advice, which urges us to consider what scientists do. According to Kuhn, "no process essential to scientific

I profited greatly from the encouragement and criticisms of Charlene Entwistle and Professors Steven Sanders and Robert Fitzgibbons.

development can be labeled 'irrational' without vast violence to the term." By examining the practice of science we will find what distinguishes astrology from science, namely, that astrologers "had no puzzles to solve and therefore no science to practice."[2] Kuhn argues that puzzle-solving, from recording more precise observations to determining the formal relationships of a theory, is the essential mark of a science. Kuhn argues further that astrology lacks such a mark. However, Kuhn too fails to shut out astrology. For astrology boasts of an abundance of puzzle-solving, from determining more precisely the time of birth to including other constellations besides "the twelve."[3]

Although Popper and Kuhn may be correct in seeing astrology and company as irrational, they do not show them to be so. These "disciplines" may not *rationally* falsify, or may not *rationally* solve puzzles, but they do engage in falsifying and puzzle-solving and confirming and whatever other general methodological strategies we may wish to add. To show that they do not rationally falsify or solve puzzles or whatever would demand an in-depth scrutiny of these areas, and *that* is a scrutiny neither Popper nor Kuhn cares to make.

Hence we must look to more than just a slogan if we wish to expose an area of study like astrology. So let us now consider briefly another type of critique against astrology, one that addresses its content rather than its method. We shall see immediately that this approach, dealing in carefully developed argument, does not fall into the trap of the quick dismissal. Consider then the following arguments.

1. Astrologers tell us that the stars influence our lives, especially the twelve constellations along the ecliptic—the apparent annual path of the sun through the heavens. Astrologers determined this path by observing the sun as it set and placing the sun "in" the constellation that supplied its backdrop. Why they chose sundown rather than some other time to place the sun "in" a constellation is obvious; it was easier. They chose not to place the sun at sunrise because few wished to rise that early. They chose not to place it at noon because it would be difficult to remember just where the sun was (at noon) and then to figure out the position of the stars (at noon) by calibrating their counterclockwise drift. Hence that the sun is "in" Aries, say, seems to be attributable to human sleeping habits rather than to any cosmic reality.

2. Moreover, the sun is not "in" these constellations an equal amount of time. It spends, for instance, forty-seven days in Virgo but only six in Scorpio. So to allow the poor Scorpios equal time, astrologers divided the ecliptic into twelve equal compartments of 30 degrees each, called them the "signs of the zodiac," and henceforth dealt directly not with the stars but with the signs—signs that have at best a rough-and-ready connection to the initial constellations. Apparently the signs, originated by astrologers to give equal time to the Scorpios and their neglected brethren, can mediate the powers of

the stars themselves. Or do the signs now have the Force?[4]

3. Worse still, the signs fluctuate. In the first century B.C. the sun "entered" Aries on March 20, the time of the vernal equinox, and since then astrologers as a rule have stated that the sun is "in" Aries during this time. However, as Hipparchus observed in 130 B.C. and Newton explained in 1687 A.D., the equinox itself is moving. Every 2,140 years the equinox slips back one sign, so that today the sun "enters" Aries not in the latter part of March but in the latter part of April. Hence what was a Taurus two millenia ago is now an Aries; what was a Leo is now a Cancer. Apparently there occurs a cosmic shift of character every two millenia, or the original signs (which mediated the constellations) are now themselves mediated by the "new" signs.

4. Furthermore, astrology offers no account, or even the promise of an account, of itself. To explain the infuence of the stars—or of the shifting signs—in terms of emanations, waves, or whatnot seems to be no more than obfuscation. For unlike the mathematically denoted entities of physics, such as Pauli's neutrino, the alleged emanations of astrology have never been experimentally identified. Moreover, to appeal to "the principle of correspondence," which tells us "as above so below, as below so above"—for example, as Mars above is red so Mars below means blood and war—is to commit a common fallacy of false cause and so to move from any attempt at science to mere magic.[5]

5. We can finally note that statistical studies have repeatedly shown the predictions of astrology to be false. A study of the *American Men of Science*, for instance, revealed no unusual groupings. Nor did an examination of 2,000 famous artists and painters find any preference for Libra. Nor again did a look at 7,000 persons who were connected with violent death see Mars or Saturn as ascendant.[6]

What can we say of arguments 1 through 5 above, and of similar arguments concerning content that we might present? As a group these provide excellent reasons for us to reject astrology, in the sense of rejecting astrological claims as confused, unsupported, or false. But surely we want to say more than that we have reason not to be believers. We want to say that no one today *should* adhere to astrology. We want not just to criticize it but to dismiss it, and to show that those who uphold astrology are simply irrational.

Yet no matter how devastating such a critique as the above may be, it cannot show that astrology or any kindred area is irrational. For in science and elsewhere we repeatedly find similarly severe criticisms. Consider contemporary physics, for instance, which offers not only the incoherence of the wave-particle dualism—where the fundamentally real is held to be in a specific place and interacting by local contact (like a particle) and yet to be spread out over an area and interacting by interference (like a wave)—but also a plethora of unrelated and so seemingly chaotic microparticles, red-

shifts that cannot be aligned with quasars, and so on. Yet it would be silly indeed even to suggest that we dismiss physics or our various physical theories as irrational.

The point is that all positions, even our own, face severe criticisms. And all positions but our own we find to be false and even absurd. But that we find positions in such straits hardly condemns them as irrational. For to find a view irrational is to disallow argument. It is to say that argument with *them* is foolhardy or a waste of time and breath. Consequently to show that we have good reason to reject a position is not at all to show that the position is irrational. Most of the time it is an invitation for further discussion. But such an invitation is precisely what we do not want to tender astrology and its ilk. We want to say of them that it is a waste of time to discuss "the issues."

We have so far investigated two types of criticisms of astrology, of method and content, and have rejected both—the first because no formula can portray (ir)rationality, the second because major difficulties plague all positions and do not by themselves signify irrationality. At this point it seems clear that a critique of the content of astrology, no matter how damaging, will never be able to show irrationality. Indeed such a critique is a way of conferring respect; we take an area of investigation seriously enough to consider the ideas involved with careful attention. But we want to say more than that we have good reason for not heeding astrology. We want to say that astrology and company should not be taken seriously at all.

To say this we must once again turn to the methodology of astrology. Before we embrace anarchism in knowledge, where astrology and astronomy fare the same, let us once more look at how astrologers reason with regard to their study. But this time let us not look with a formula in mind, with the aim of a quick dismissal, but instead with a more careful attention to the complexities of reasoning. This means, alas, that we will have to read or listen to what astrologers say regarding astrology. But we will find that the dreariness of the study itself supplies a crucial clue to what lies behind irrationality in general and the irrationality of astrology in particular. For to listen to astrologers speak as astrologers is to reject them, not because we fail to understand them, and definitely not because what they say is bizarre—after all, contemporary physics is unmatched in the region of the strange—but instead because we understand them all too well and because what they say is so common.

Consider the following points concerning what passes for respectable reasoning—argument, falsification, puzzle-solving, self-reflection—in astrology. These points, by the way, do not merely concern streetcorner and newspaper astrologers, the bane of the "serious astrologer." They concern the allegedly respectable astrologers—the recognized leaders of the field—who hold that in a "proper, ordered society the astrologer should be in much the

same position as doctor, psychologist, or priest."[7]

1′. Had there been no Common Fallacy List as supplied by logic texts, we would have had to invent one just to deal with the various fallacies endemic to astrology and kin. In just a few pages of a typical work[8] we go from the standard appeal to authority, this time to Kepler, "one of the last of the great astronomer-astrologers"—as if being an authority in the past and in one area magically carries over to being authoritative in the present and in another area—to the usual vague claim: "One of the most striking pieces of evidence in favor of astrology is the fact that those born at the same moment of time in approximately the same place have a life pattern that is very similar." What, we might well ask, is "the same moment" in an Einsteinian world? And what qualifies as "the same place" on a rotating and revolving planet: Massachusetts? Boston? Beacon Street? 325 Beacon Street? In the same few pages we meet the egregious analogy: "The construction of the atom bears a close relationship to the Solar System"—again set in a quantum-mechanical world that exploded such a notion decades ago. For the record we might note one more fallacy, a mundane reliance on false cause:

> On the day Queen Wilhelmena of the Netherlands married only one other woman in the country was allowed to marry. She was a friend of the Queen, her name too was Wilhelmena, and she was born on the same day as the Queen.

The inference here is all too easy to parody. Scotch and water makes me drunk, bourbon and water does too, and again Canadian club and water. So clearly water makes me drunk.

This is typical of the literature of astrology. Here we meet no subtleties in argument, as we do with Copernicus or Galileo, but a dull series of mismanagements of even the simplest considerations. Is it any wonder that such stalwarts as Popper and Kuhn shrink from contact with such stuff and search for an easier way—a slogan, a formula, anything?

2′. When we criticize astrology we find the replies trite and shallow, not worthy of close attention. We have already seen one attempt to reply to a criticism in the so-called principle of correspondence. Consider also this reply to the hoary objection that astrology cannot account for the twins who lead different lives. We are told that twins sometimes do

> look and act alike, marry at the same time, have the same number of children, die on the same day, etc. How can we explain this except by astrology? To explain the other possibility [where they lead different lives], we have only to take into account the differences in heredity and environment.[9]

The astrologer would have us explain the similarities by astrology, the dissimilarities by heredity and environment. But what justification could there be for this neat bifurcation of duties? Nary a word do we hear of the

possibility of a unified view through genetics or even a note concerning the arbitrariness of this division—unless we count the expostulation that such a problem "is so complex and so unpromising that most astrologers prefer to admit their incompetence in the matter and get on with their work." [10] Hey, don't trouble me with *argument*, I'm busy.

3′. Furthermore, when astrologers do come upon an interesting consideration in their favor, they make no attempt to develop it—or in Kuhn's terms, to engage in "puzzle-solving." For instance, the non-astrologer Gauquelin researched 25,000 births and found a pronounced correlation between the planetary positions and general occupation. More doctors seemed to be born when Mars or Saturn was rising or had reached its culmination, for example, than would have been predicted by chance alone. [11] Since then astrologers have referred to this work as final: "Gauquelin's work proves once and for all, and incontestably, that there is *something* to astrology." [12] From the context it is amply clear that this "something" is close to if not the whole truth. Other possibilities are left unexamined and even unmentioned—for example, that children may tend to be born at the same time as their parents and also tend to follow their parents' interests (especially in Europe), or that Gauquelin may have erred in his statistics. Instead we are told emphatically that astrology has been incontestably demonstrated. That reasons bear relative weight, that statistics are multiply edged—well, most astrologers are apparently willing to admit their incompetence in the matter and get on with their work. That their work should involve knowledge of statistics and the like is of course irrelevant to raking in money.

4′. We might finally note that one of astrology's failings is its penchant for offering specific directives. Astrology sees that the social sciences do not tell us when to marry, to make a major decision, to take a chance. Thus it advertises itself as filling a crucial gap, as doing what the social sciences either cannot or refuse to do. Yet it is in this very reticence of the social sciences that we meet an important mark of rationality. For rationality does not mean that everything is explained, all tidy and neat, but instead demands a recognition of the limits of explanation and theory. These limits range from the statistical point, that our theories predict the behavior not of individuals but only of a proportion of a group, to the conceptual point, that our concepts are vague, lack sharp boundaries, and so cannot always say what is included in what. Hence the coincidental, which astrology has ostensibly declared war on, turns out not to be a failure of thought, a surrendering of explanation—as portrayed by astrology—but a consequence of the very logic of our systems. Our concepts can only be stretched so far and can only cover so much. In seeking for a life recipe, in other words, astrologers show no appreciation for the limits of reason.

Faced with 1′ through 4′ above as a typical collection of fallacious moves and dubious maneuvers, what can we do? Where do we begin? So much is

wrong, so much is off, that it is doubtful whether any critique would help. For the critique would likely be understood only by those who do not make such a mass of errors in the first place. Do we speak of false cause? But first we must lay the groundwork by developing the notion of causality and lawful generalizations and the like. Do we speak of unifying our theories? But then we must discuss the criteria of coherence and simplicity, their relative weights, how they fit in with other criteria, and so on. Do we address the problem of developing our ideas through puzzle-solving or the notion of the limits of explanation? But think again of how much is assumed in these rather basic ideas.

To show that astrology is irrational, accordingly, is to tell a long story. It is to take what we have stated above in 1′ through 4′ as topic sentences and follow through these and related ideas to their full development. It is to read the woeful passages of what passes for argument in works of astrology and expose the myriad patterns of specious reasoning for what they really are. We know, perhaps, what we mean by argument, falsifying, puzzle-solving, and the like. But so much, indeed most of it, is implicit, signifying the past mastery of myriad techniques, standards, qualifications, and so on.

Astrologers thus do not miss out because they lose the "rationality race" but because they fail to qualify for and so fail to enter the race at all. Rationality involves mastering no one procedure—whether it be falsifying, puzzle-solving, or whatever—but, like qualifying for other matters, demands a mastery of a host of procedures. It does not demand that we be right, that we win, but that we be competitive—as revealed by our previous races and showings of competence. Hence to fail to do well in certain previous races does not by itself mean that we fail to qualify; for there are many races involved in the final qualifying decision. But what if one comes in last race after race, or frequently runs the wrong way? What if throughout astrological writings we meet little appreciation of coherence, blatant insensitivity to evidence, no sense of a hierarchy of reasons, slight command over the contextual force of criteria, stubborn unwillingness to pursue an argument where it leads, stark naïveté concerning the efficacy of explanation and so on? In that case, I think, we are perfectly justified in rejecting astrology as irrational.

Our initial question, How do we know that astrology is irrational? turns out after all not to be complex. We do know. Astrology simply fails to meet the multifarious demands of legitimate reasoning.

This, to be sure, is vague. But rationality is a global notion, one embracing a legion of criteria, procedures, techniques, asides, qualifications, and the like. To insist on a precise definition is to misunderstand it. Rationality could be characterized as the appropriate, correct, and self-critical openness to ideas. We reason appropriately when our reasons fit the situation, correctly when they are valid and sound, and self-critically when our ways of reasoning

are themselves scrutinized. And similarly irrationality could be characterized as the pervasive lack of the appropriate, correct, and self-critical openness to ideas. But the necessary generality of any such characterization is soon seen as the real message. In the end this only tells us that rationality involves, well, legitimate reasoning—and irrationality, illegitimate reasoning.

Yet we do not lack clear ideas regarding the various constituents of rationality. It is a gross understatement to assert that we often know when an argument is valid or invalid, when it should be or has not been qualified, what sort of considerations are or are not suitable.

We can be said to know that astrology is irrational not because of our advertised critiques but in spite of them. We know for so many reasons—or for so many failures of reasons. But we hardly want to be troubled to chronicle all of these failures. For as we have seen this would involve developing so many elementary ideas and rudimentary techniques and methods—all of which has been done before, by others—that it would in no way be time well spent. Hence we economize and advertise a neat and quick dismissal, one that appears to do the job as long as we do not examine it too carefully.

Nor is this to say that rationality is just a matter of common sense. Rationality must be learned—if we are to do it well. Rationality may have no essence, a formula to learn and practice as we did the multiplication tables, but it still must be taught and learned. Indeed, even more so. For mastering the full complexity of rationality is a task that is inherently incompleteable—as well as one that astrologers and their kin have hardly begun. For their arguments are as if Plato and Aristotle and Lucretius and Aquinas and Galileo and Locke and Hume and Planck and Einstein and Russell and Wittgenstein and, indeed, Popper and Kuhn, and so on, had never been. Moreover, that most astrologers would grasp this list with trembling and irate hand, charging that Plato and Aristotle and Aquinas were not adverse to their star study, only betrays how far they have to travel. If the simple fallacies like appeal to authority escape them, what can we expect when the analysis gets tough?

If this sounds harsh, well, so be it. For there are not that many areas of study that can be rejected as irrational. Since rationality is such a complex notion, it admits of varying degrees and forbids sharp lines (as found, say, in slogans). To commit a fallacy or two, to be caught in a contradiction, to fail to pursue a consequence, and the like do not by themselves condemn one to irrationality. Howlers happen to us all. It is only when we encounter one piece of bogus reasoning after another, as we do with astrology, that we have a clear case of irrationality.

Of course none of these considerations will succeed in abolishing astrology and its companions. Although astrologers do appear silly when compared to thinkers who reason well, not all care to compare, let alone strive to attain

such standards. Moreover, since rationality lacks an essence, the one standard that must be met, it is easily counterfeited. When there is no one lack that we can point to as decisive, when we cannot dismiss astrology's gibberish by a word, few care to tell or listen to the required tale. For then we must read the astrological literature. Unless we are slumming or looking for another instance of fallacious reasoning, wading through such muck offers no pleasure. And woe, even after we do take the time to point out so many foolish flaws, other astrologers arise, like mythic armies of yore, claiming to have swept out their houses. Although a cursory look on our part reveals the same dreary deal, the thought of grading such remedial reasoning again—and again and again—is overwhelming.

Hence it should be no surprise that we tend to follow Popper and Kuhn and look for a quickie dismissal, one that suffices for the already converted but does not approach what we really need to say. A wave of the hand, a clever aside, may protect a tired psyche but hardly suffices to reject the irrational. For that requires an extended, nay, an indefinitely long series of analyses, war after war, and all along we have been treating it as a skirmish.

Notes

1. Karl R. Popper, "Science: Conjectures and Refutations," in *Conjectures and Refutations* (New York: Basic Books, 1962), and included in this volume.

2. The first quotation is from Thomas S. Kuhn, "Reflections on My Critics," in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (New York: Cambridge University Press, 1970), p. 235; and the second is from Kuhn, "Logic of Discovery or Psychology of Research?" *Criticism and the Growth of Knowledge*, p. 9 and included in this volume.

3. For instance, for a discussion on new constellations see Owen Rachleff, *Sky Diamonds* (New York: Hawthorne Books, 1973); or for the theory of subsumption, see Henry Weingarten, *A Modern Introduction to Astrology* (New York: ASI, 1974), p. 65.

4. For more on 2 and 3 see Isaac Asimov, *Matters Great and Small* (New York: Doubleday, 1975), Chs. 3 and 4.

5. Cf. Lawrence E. Jerome, "Astrology: Magic or Science?" *The Humanist* 35, no. 5 (September/October 1975): 10–17.

6. Cf. Michel Gauquelin, *Astrology and Science*, trans. James Hughes (London: Peter Davies, 1969), pp. 132–146.

7. Ronald C. Davison, *Astrology* (New York: Bell, 1963), p. 169.

8. Ibid., pp. 9–13.

9. Weingarten, p. 66.

10. John Anthony West and Jan Gerhard Toonder, *The Case for Astrology* (Baltimore: Penguin Press, 1973), p. 148.

11. Gauquelin, pp. 161–171.

12. West and Toonder, p. 172.

Spheres of Influence

MICHEL GAUQUELIN

"You shouldn't dismiss as incredible the possibility that a long enough search might reveal a golden grain of truth in astrological superstition," was the advice of the great Renaissance astronomer Kepler. In all modesty we might claim to have proved Kepler right when we describe our work and the surprising results we have obtained. For I believe that I have shown a link between the movements of the planets and human character and career.

In 1953, we had just finished a major inquiry into astrology. We had subjected its laws to statistical tests and all the results remained negative. There seemed to be no truth in the idea that signs of the zodiac determined anything or that horoscopes could be used to predict the future.

When, however, we came to examine the position of the planets at the hour of birth of 576 members of the French Academy of Medicine, something very odd showed itself. Among these distinguished doctors, certain planetary positions appeared at the moment of birth far more than they should. This result could not be dismissed as "mere chance." Any statistician would have judged it to be very significant—which is what we did.

What we observed did not fit any traditional law of astrology exactly. Famous doctors seemed to have a peculiar preference for being born when either Mars or Saturn had just risen above the horizon or, second, when either planet had reached its high point in the sky (its culmination) for that day. Ordinary mortals seemed to differ. We took care to examine the hours of birth of perfectly ordinary people taken at random from electoral rolls. They showed no preference for timing their birth according to either Mars or Saturn.

All this needed to be examined in greater detail and, if possible, explained using a sound methodology. It is worth stressing again that these findings clearly had nothing to do with signs of the zodiac.

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A Day in the Life of a Planet

Because the earth rotates round its axis every 24 hours, the heavenly bodies—the sun, moon, planets, constellations and stars—seem to rise in the east. Each body then climbs to its culmination, its high point in the sky for that day, before coming down, like the sun, to set in the west. The planets follow this pattern. Every day, each planet rises above the horizon close to the time it did the day before, climbs in the sky and then sets.

To apply probability theory to the positions of the planets we divided the circle of the daily orbit of each planet into twelve, eighteen and thirty-six sectors. The more sectors, the more detail one can obtain, but the methodology remains the same in all three cases. In this article the results we present are based entirely on division into twelve sectors. We have at our disposal, therefore, a kind of cosmic roulette with numbers running from one to twelve. When a child is born, each planet in the solar system is in one of these twelve sectors. If we examine 100 or 1,000 births, we can count the number of times that Mars lies, for example, in sector one, sector two and so on, to sector twelve.

20,000 Famous Births

Our data on doctors who were members of the French Academy of Medicine encouraged us to gather data on the times of birth of more successful people in various fields. We looked at scientists, actors, writers, sports champions and other professionals. Success may seem an arbitrary criterion but it is an objective one. It is relatively easy to agree whether or not a person is successful. This also allowed us to examine people in each profession who clearly showed the psychological characteristics that marked them for success in it.

Celebrities are also useful because they are well documented in dictionaries of national biography, *Who's Who* and a host of other reference works. In these we found many dates of birth, although the hour of birth is not—alas—usually noted.

We therefore got in touch with the Registrar's office at the place of birth of each of our subjects. In many countries, including France, Italy, Belgium, Holland and Germany, the hour of birth is automatically recorded. (In England and Wales the time is noted only in the case of twins. It is different in Scotland!) Our total number of subjects was over 20,000 European notables including politicians, artists, scientists and actors. In 1970, our laboratory undertook to publish in six volumes the hours of birth and corresponding planetary positions of each of these 20,000. This will enable all interested scientists to verify our data and conclusions.

Timing Your Destiny

The more calculations we performed, the more the initial trend was confirmed. Surprising as it may seem, a more and more precise statistical relationship appeared to emerge between the time of birth and great men and their professional success. It was no longer just a matter of the doctors of the French Academy of Medicine. Each professional group seemed to have a planetary "clock" all its own. This basic observation seemed to hold especially good for the moon. Mars, Jupiter, Saturn and Venus showed the trend to a lesser degree.

It was almost as if, for certain occupations, the presence at birth of a planet which had either just risen over the horizon, or reached its high point in the sky, seemed to "provoke" or "cause" success. Among 3,647 scientists, 704 were born after the rise (sector one) or after the culmination (sector four) of Saturn. By chance alone one would expect 598. The odds against the number we found are 300,000 to one. Mars dominates a sample of 2,088 sports champions. 452 were born either after it rose or culminated. Chance predicts 358 should be. The odds against: five million to one.

Soldiers and Jupiter

Among 3,458 soldiers, Jupiter is to be found 703 times, either rising or culminating when they were born. Chance predicts this should be 572. The odds here: one million to one. There is a lesser, but similar, trend observed for births to occur when the planets are in sectors seven and ten—just after the setting and at the lower culmination, the low point of its path.

The effect does not always seem to be extended on the *plus* side. Sometimes, the presence of a planet just after it had risen over the horizon, or just after it had culminated seemed to "prevent" success in certain professions. Only 203 of 1,473 great painters were born with Mars in sectors one or four. The odds against this: 200 to one. Only 287 writers and journalists were born with Saturn in the same position. The odds against this: 300 to one. Table I shows the essential data; figure 1 gives details for Jupiter.

Other Research

All manner of scientists responded sceptically to these astonishing results. Statisticians, psychologists, astronomers, demographers, biologists and even midwives took pains to explain it away. They argued that we might have been biased in choosing our births, mistaken in our use of statistics, incompetent in handling astronomical details. But the evidence didn't budge.

The "resistance" of these inexplicable relationships intrigued the grandly titled Belgian Committee for the Scientific Investigation of Phenomena Reputed to be Paranormal. This "Para-Committee," based in Brussels, decided to repeat our observations. The Committee is made up of thirty

TABLE I. Professional groups, with an abnormal frequency (higher or lower) in the number of births before the rising and culmination of the planets (sectors 1 and 4):

Planet	Profession	No. of births	Frequency observed at rising and culmination	Theoretical frequency at rising and culmination	Divergence	Possibility of chance 1 in:
Mars	Science and Medicine	3647	724	626	+ 98	500,000
	Sport	2088	452	358	+ 94	5,000,000
	Army	3438	680	590	+ 90	1,000,000
	Painting	1473	203	253	− 50	200
	Music	866	120	149	− 29	30
Jupiter	Army	3438	703	572	+ 131	1,000,000
	Politics	1003	205	167	+ 38	100
	Theatre and Cinema	1409	283	235	+ 48	1,000
	Journalism	903	185	150.5	+ 34.5	100
	Science and Medicine	3647	602	540	+ 62	30
Saturn	Science and Medicine	3647	704	598	+ 106	300,000
	Painting	1473	188	238	− 50	200
	Literature and Journalism	2255	287	338	− 51	500
Moon	Literature	1352	292	225	+ 67	100,000
	Politics	1003	189	167	+ 22	20

For the calculation of theoretical frequency, the exact astronomical definition of the positions of the zones after rising and culmination, and the actual values obtained, please refer to M. and F. Gauquelin, vol. 1, Series C, Statistical Results of the Series A and B (Laboratoire d'etude des Relations entre Rythmes Cosmiques et Psychophysiologiques, Paris, 1972.)

scientists of different disciplines. Since it was set up 20 years ago, it had often denounced false correlations especially where the influence of the stars was being studied. The Committee is feared by those who believe they have discovered new facts since, often, the Committee's attempts to verify a new fact have revealed nothing more novel than a mistake. The Committee was very hostile to our conclusions but decided in 1968 to repeat one of our most significant studies on a new set of new subjects. They chose sports champions. They assembled a different group of 535 champions and a computer was duly fed all the necessary details about the hours of birth of these sportsmen and with all the complex movements of Mars.

The statistical evaluation of the position of Mars at the time of birth of these 535 sportsmen showed, once more, that the planet was either rising or culminating when they were born far more often than one would expect by chance. The Committee noted the phenomenon with surprise and judged that it posed a real scientific problem. It also expressed doubts as to what

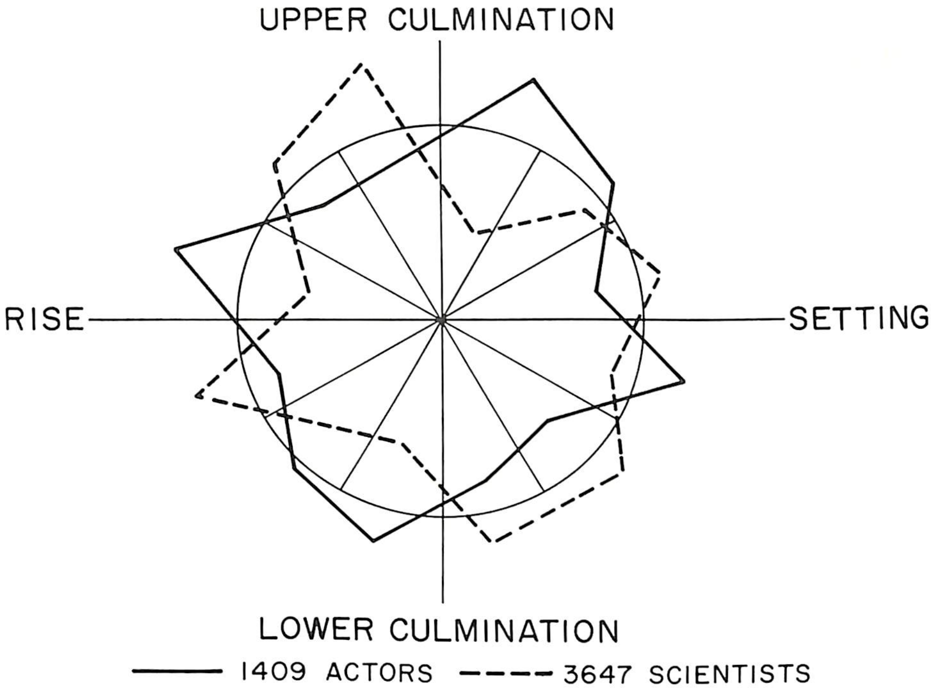


Figure 1. The position of Jupiter at the birth of actors is very different from its position at the birth of scientists. The times favouring the birth of successful actors (continuous lines) correspond to the times that "prevent" the birth of scientists (broken lines). The circle indicates the theoretical frequency for ordinary births. The effect is statistically remarkable.

might have caused it. Despite a welter of critical studies and the use of exceptionally refined controls, the fact persists. (Table II)

TABLE II. The planet Mars in its daily movement at the moment of birth of sports champions.

A Sectors	1	2	3	4	5	6	7	8	9	10	11	12	Total
Observed	172	126	128	160	117	104	126	125	132	112	118	133	1,553
Expected	138	135	133	128	126	124	122	122	127	130	134	134	1,553
B Sectors	1	2	3	4	5	6	7	8	9	10	11	12	Total
Observed	68	47	35	52	35	31	36	51	53	53	40	34	535
Expected	48	47	45	44	43	43	42	42	43	45	46	47	535

Mars is much more frequent than usual in sector 1 (after rising) and in sector 4 (after culmination):
A. Our investigation of 1553 sports champions (332 against 266, significant to a degree of .001)
B. The investigation carried out by the "Para-Committee" on 535 other sports champions (120 against 92, significant to a degree of .01)

Personality and Success

How does one explain these facts? Psychological factors underlie everybody's success. Success at the highest level in a profession reveals both personal taste and a powerful vocation. There is a psychological connection between personality of individuals and their success. Many psychologists have noted this. Character is an important part of success. And each profession has a typical psychological profile.

We sent out a questionnaire to examine these characteristics of professional groups. University-educated subjects were asked to specify which characteristics or personality traits seemed to favour and also to handicap those who sought success in science, art, letters, politics and the military. The analysis of replies allowed us to frame a psychological profile for each profession. Scientists are, for instance, scrupulous, self-absorbed, timid, quiet, discreet, prudent, cautious, stubborn and methodical. Sports champions are persistent, courageous, energetic and bear hardship well. "Captains of Industry" are realistic, hard, dominating, authoritarian and well organized.

This image of traits is confirmed by psychologists who have looked at the link between personality and professions. Anne Roe studed a small number of selected scientists, all of whom were distinguished. She found that the physicists and biologists tended to have had an isolated childhood. They dated little, had a restricted social life and few contacts with others.

Skilled and Supple Sportsmen

In his work on the psychology of sport, Michel Bouet has written of champions: "Sport does not merely call on their physical attributes of speed, endurance, skill and suppleness, but also calls on their will, courage and persistence." The French psychologist, Claude Levy Leboyer, writing of important business men in his study of success, argued: "Successful businessmen reveal traits of character which one does not find among famous writers or scientists. These include the ability to make rapid judgements, a high sense of realities and a need for constantly driving themselves (a new goal is set the moment an old one is reached)."

To return to the planets. Two factors set scientists apart. They are usually introverted and their times of birth are unusually linked to various positions of the planet Saturn. Hypothesis: there is a relationship between Saturn and the classic personality of the scientist. Sports champions, for their part, are energetic, brave and their times of birth seem unusually linked to Mars. Is there, in this case, a relationship between the planet Mars and the personality traits that go to make a sports champion? One could repeat the hypothesis for other groups–namely that what these planetary oddities reveal is not luck or

fate but the workings of various personality factors which have been influenced by the planets.

Collecting Individual Traits

To establish this hypothesis, one has to examine many individual cases to show the link between the planets and personality.

We proceeded like this. We used the same set of people as before. This time, we collected their biographies. In 1968 we devised a way of using biographies objectively. We based our method on the idea that there are permanent traits of character such as that defined by J. P. Chaplin in his *Dictionary of Psychology*: "a relatively persistent and consistent behaviour pattern manifested in a wide range of circumstances." A short example may be useful. This appreciation of the Nobel prizewinning writer, François Mauriac, appeared the day after his death:

> One of the dramatic features of his long career was his impetuous curiosity which made him turn to journalism where he soon made his name. Irritable—and irritating for others—he wrote ferociously, impulsively and indefatigably. He made his *Bloc-Notes* [a literary diary that appeared weekly in *The Figaro*] into an institution. He expressed in them his enthusiasms and his hates directly. His political activity was the product of his whims and passions. Such a career made François Mauriac a 'character' of letters. His books were awaited, his sayings quoted. His attacks were themselves attacked and he responded to these and the whole battle received much official notice. The long emaciated silhouette of Mauriac became one of the images of Parisian life. He liked the theatre, the music hall and the atmosphere of premieres. His body had become bent with age. His voice became thick and harsh as a result of a painful illness, but this seemed to point rather than lessen the words which poured out, and the enthusiasm or anger that he expressed lacked no clarity.

This small, vivid portrait on its own is useless for a statistical analysis. We therefore underlined all the traits of character and behaviour attributed to Mauriac. This impoverishes the portrait but it does make it possible to pool it with others in a large statistical analysis. For, by reducing the portrait to each mention of a trait, we obtain the following version of the writer: "Curious—impetuous—irritable—ferocious—impulsive—indefatigable—enthusiastic—cruel—expresses himself directly—prey to whims—passionate—'a character'—coins sayings—lively—loves theatrical life—easily angered—enthusiastic." For each trait we opened a file, carrying on one side Mauriac's name, and on the other the position of the planets when he was born.

What we did for Mauriac, we repeated for other celebrities, using published accounts of their personality. This gives us files on thousands of personality traits taken systematically from biographies. To ensure the objectivity of the study we have to:

(i) achieve a certain unity in all this biographical material.

(ii) never eliminate a biography because it fails to fit our hypothesis.

(iii) consider every trait mentioned in every biography.

A computer programme at the University of Paris's Faculty of Science allowed us to analyse the data collected as objectively and minutely as possible. Table III gives a short extract of one of the listings obtained. The study looked at one professional group after another. The data is published by our laboratory in our *Psychology Monograph Series* and each monograph gives, in English, a description of the method used, the statistical results, the catalogue of all the personality traits and, in full, all the biographical references from which these were culled.

TABLE III. Relationships between the position of the planets and character traits.

Name	Character	Frequencies	Moon	Venus	Mars	Jupiter	Saturn
Busnel, R.	Active	1	5	28	30	20	6
Codos, P.	"	1	30	20	24	10	35
Faucheux, L.	"	2	11	2	32	30	27
Fournier, A.	"	1	5	27	12	29	14
Gauthier, B.	"	2	24	21	2	11	16
Gerardin, L.	"	1	24	23	22	17	35
Gerbault, A.	"	1	33	36	11	26	12
Graule, V.	"	2	1	11	2	1	4
Hilsz, M.	"	1	33	21	3	26	25
Anglade, H.	Adaptable	1	8	22	18	19	3
Batteux, A.	"	1	18	19	26	22	19
Bastien, J.	Skillful	1	33	9	11	16	7
Batteux, A.	"	1	18	19	26	22	19
Deglane, H.	"	1	23	9	8	19	22
Drigny, G.	Affable	1	23	25	29	22	28
Rimet, J.	"	1	3	11	35	12	34
Dewaele, M.	"	1	33	22	9	1	29
Thoret, J.	Nervous	1	6	11	21	7	24
Michard, L.	Agreeable	1	18	19	9	3	6
Bigot, J.	Aggressive	1	21	36	10	25	12
Famechon, E.	"	1	5	32	2	9	6
Codos, P.	Bitter	1	30	20	24	10	35

Examples for sports champions. From left to right: the name of the champion, the character trait attributed to him, the number of times that the trait has been attributed to the champion, the position (from 36 sectors of the daily movement) at birth of the moon, Venus, Mars, Jupiter, and Saturn.

The Martian Personality

We assembled many biographies of sports champions from works like *The Dictionary of Sports* and sporting weeklies edited by sports specialists. They give not just information about performances and records but also indications as to personality. The 6,184 character traits attributed to these individuals were each filed on a card alphabetically. With each trait is also filed Mars's position at the birth of the individual who has the trait.

We then looked at the literature on the psychology of sport. We used especially the work of M. Vanek and B. Cratty, *Psychology and the Superior Athlete* (Macmillan 1970), and M. Bouet, *Les Motivations des Sportifs* (editions Universitaries 1969). These allowed us to establish a character image of a typical champion. Here are some of his attributes: energetic, dynamic, strong-willed, brave, hard, tireless, lively, iron-willed, persistent.

We examined our list of champions. Each time we noted a trait which had been listed, we saw where the planet Mars was at the time of birth of that particular champion. If Mars is over the horizon, and after culmination is linked with becoming a sports champion, then one can argue that the influence of Mars ought to be even stronger on those sportsmen whose personality approaches the typical sportsman's character.

This prediction was confirmed. Take the traits *brave* and *strong-willed.* Mars is either rising over the horizon or, more often, just culminating at the birth of champions who have these traits. The excess of Mars, so to speak, is so great that the odds against it happening by chance are ten million to one. If one chooses sportsmen whose characteristics are untypical—calm, lazy,

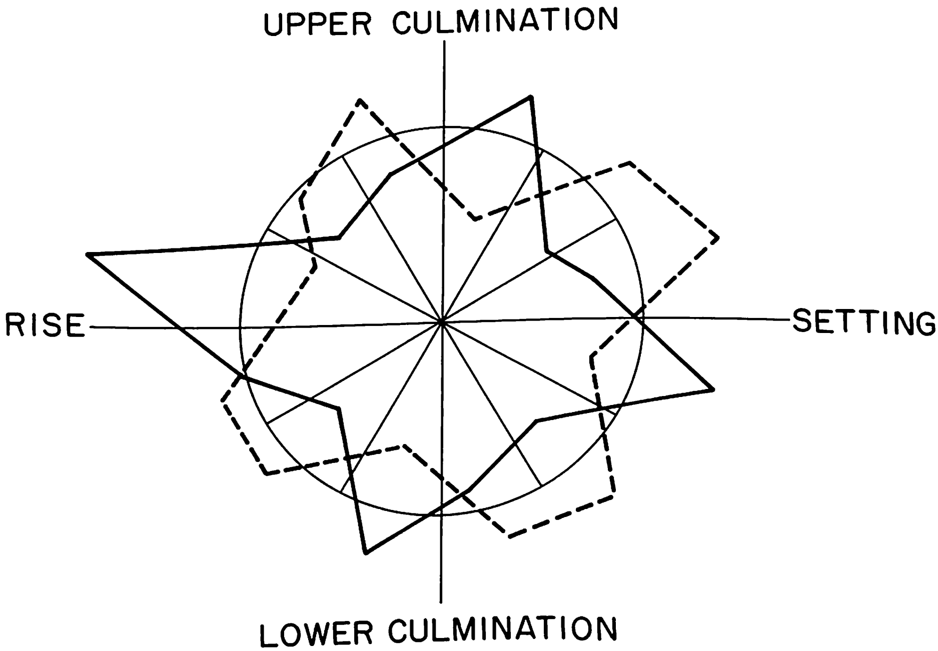


Figure 2. Mars and Strong-willed and Weak-willed Champions
Circle: the mean number of champions born with Mars in each part of its daily orbit.
Continuous lines: strong-willed champions are born more often just after Mars has risen or culminated.
Broken lines: weak-willed champions are born less often with Mars in these positions.

nonchalant—not many of them were born when Mars was either rising or culminating. The position of Mars at the birth of strong-willed champions is utterly different from the position of Mars at the birth of weak-willed champions (see figure 2). One last statistic: Mars appears in sectors one, four, seven and ten in 64 percent of the births of strong-willed champions as against 33 percent for the average man. For sports champions lacking the iron will Mars is present at 29 percent of births. The position of Mars is the expression of a personality factor. Only incidentally is it related to professional choice.

The Saturn and Jupiter Temperaments

We conducted a similar study on well-known actors. This time we examined the personality traits connected with the planet Jupiter, which is linked with actors' births more than expected by chance (see Table I). Psychologists have listed the following among the most noted characteristics of actors—elegant, immodest, exuberant, funny, at ease, wastrels, boasters, theatrical, eccentric and egocentric. Not all actors share this motley assortment of defects. Louis Jouvet, the great actor, suggested a distinction between exhibitionistic and restrained actors. The latter were rarer and possessed quite opposite traits. They were modest, shy, scrupulous and quite untheatrical. Thanks to the 17,960 traits that we amassed we classed actors into these two categories. The results were clear. Exhibitionist actors were born twice as often when Jupiter had just risen over the horizon or had reached its culmination.

We also divided scientists into two personality groups according to their biographies, which had yielded 9,547 traits. The first were introverted scientists who were self-absorbed and modest, and the second, extroverted scientists who were more worldly and more vain. Saturn, the planet linked to scientists, should appear more clearly among the group of introverted scientists, the model of the true scientist. On the other hand, it should have no relationship with less typical scientists. The correlations confirm this (see figure 3).

The pattern of our results is that the passage of a planet just over the horizon, or just after its culmination, is the expression of a combinant factor in the personality. We can speak of a Mars factor, Jupiter factor, or Saturn factor with some precision, thanks to the method of analysing character traits. (Table IV). We are researching the moon factor and Venus factor.

The Planets and Modern Psychology

It is a fascinating project to try and compare planetary positions with personality factors. Hans Eysenck's work has established that the personality

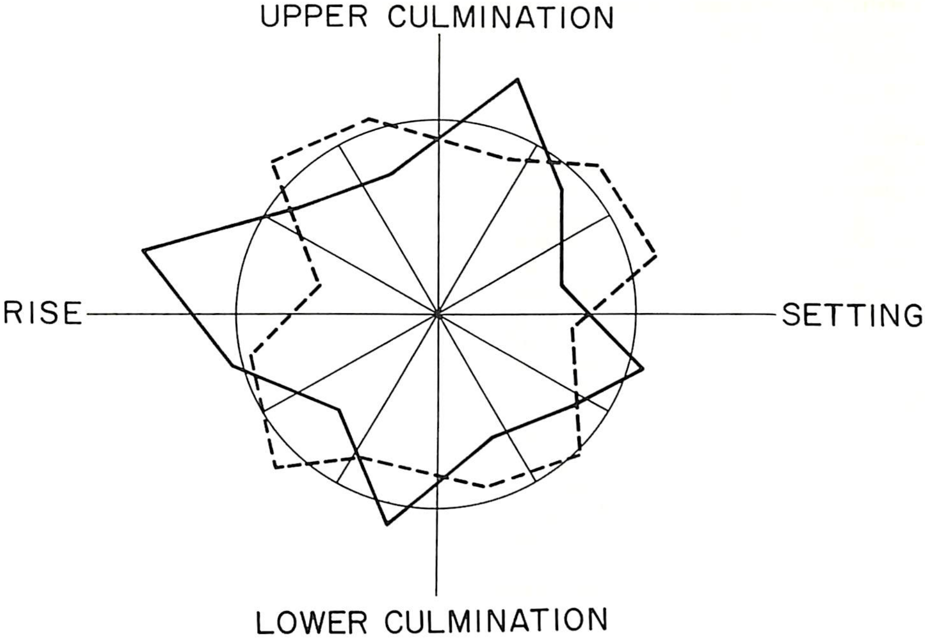


Figure 3. Saturn and Introverted and Extroverted Scientists
Circle: shows the mean number of scientists born with Saturn in each part of its daily orbit.
Continuous lines: introverted scientists are born more often just after Saturn has risen or culminated.
Broken lines: extroverted scientists are born less often with Saturn in those positions.

is structured along two bi-polar orthogonal axes: the axis of introversion/extroversion and the axis of neuroticism or stability/instability.

Our discussions with Eysenck have made it possible to devise an experimental method for linking his work and ours. We are beginning a project in which we will compare the replies of 2,000 persons on the Eysenck Personality Inventory with the position of the planets at the time of their birth. Eysenck's descriptions of introverts and extroverts make it possible to formulate precise hypotheses. Introverts should tend to be born after the rising or culmination of Saturn; extroverts should tend to be born with Jupiter in those positions. If the results are positive, they show that one can extend the findings from famous to ordinary people. It also implies that one could predict the behaviour and character of a newborn baby when one knows where the planets are at its birth—providing the birth takes place under certain conditions. We are eagerly awaiting the results.

Like Father, Like Son

These planetary influences are hard to explain and what follows may seem very controversial but one must attempt some explanation. Do rays of some

TABLE IV. Twenty character traits describing the planetary ingredients of personality:

Keywords

Mars	Jupiter	Saturn
Active	At ease	Formal
Ardent	Ambitious	Concentrated
Aggressive	Thrusting	Conscientious
Reckless	Authoritarian	Cold
Pugnacious	Talkative	Methodical
Courageous	Exhibitionist	Meticulous
Dynamic	Comedy (a sense of)	Modest
Energetic	Communicative	"Looker on"
Fiery	Coquettish	Organized
Indefatigable	Spendthrift	Speaks little
Battling	Gay	Precise
Offensive	Gestures a lot	Reflective
Afraid of nothing	Good Humoured	Withdrawn
Ingenuous	Independent	Reserved
Hardy	Joyful	Sombre (temperament)
Bold	Proud	Wise
Brave	Extravagant	Timid
Vitality (full of)	Joker	Hard-working
Vital	Sympathetic	Taciturn
Strong-willed	Vain	Sad

This provisional list of traits is an often convincing image of the personality of a person born when the planet had just passed the horizon or the meridian of the place of birth. These portraits, of course, do not exclude other influences (family, culture, and so on) which combine to make up the individual's total personality.

sort affect the infant's personality? The child is born, fully formed, with all his or her inherited potentialities. It cannot be that the planet affects the whole genetic structure at the point of birth. It seems inconceivable that planets should "add" anything that is not already innate in the child. The effect of the planets would seem more possible if its role were linked to heredity.

Work by biologists and psychologists has suggested that we inherit much of the temperamental fundamentals of our personality. The research of Kretschmer and Sheldon indicated that different kinds of physical body builds went with different personality types. Sheldon classified body builds into three main categories–endomorphism, mesomorphism and ectomorphism: Kretschmer into leptosomic, pyknik and athletic. They claimed that with each body build went a particular kind of personality. (It should be noted that their work remains controversial and not all psychologists accept it.)

In his work, Professor Hans Eysenck deals with the same theme. There is even talk of a biochemistry of behaviour with its origin in the genetic endowment each person has, since the research at the Michigan Institute and Edinburgh University showed a connection between levels of uric acid in the blood and social behaviour. If the planets give an indication of character and

personality, this must involve an inherited element. And so children must be born when the heavens are in the same shape, so to speak, as at their parents' births.

We verified this hypothesis by looking at 30,000 times of birth of parents and children. Children tend to be born when a particular planet is rising above the horizon or culminating–if that same planet was doing the same at the birth of one of their parents. Observations on this show what seems a weak effect. But given the large number of births examined, the chances of that effect appearing at random are still 500,000 to one. The chance of a child being born thus is doubled if both parents had the same planet rising or culminating when they were born. Only the moon, Venus, Mars, Jupiter and Saturn show this effect, however.

The child seems gifted with a "planetary sensitivity" which triggers the moment when he is born. The planet does not alter the genetic or biochemical make-up of the infant. It acts as a trigger for the birth when it is in a favoured position and this position in the sky serves to indicate the psychobiological temperament of the newborn. But this brings a host of new problems, for how is one to justify the effect of the planets on heredity? There is both a biological problem and an astronomical problem.

The Biological and Astronomical Unknowns

First, the biological unknown. Medical science knows that when a foetus is ready to be born, nothing is needed to trigger its birth. This propensity the foetus has to be born when it is ready might be linked with subtle cosmic factors. It is arguable that the child could react to a "sign" to which those of his heredity were more sensitive than others. This idea has the merit of fitting in with contemporary medical thinking. C. G. Liggins of the National Women's Hospital in Auckland, New Zealand, has shown that it is the foetus that cues in the moment of birth through a hormonal mechanism. Liggins' work has been discussed at a number of international symposia.

What happens, therefore, when doctors interfere with the normal process of birth, either through a Caesarian section or through induction? Here we have an obvious control group, for in these cases any planetary effect on birth disappears. Medical influence rules out cosmic influence. The position of the planets at the time of birth then gives no indication as to personality.

There still remains the astronomical unknown. How could a planet take it upon itself to affect the actual times of birth? It might be something like a catalyst, acting perhaps through a screen in the solar field. A recent experiment has allowed us to confirm that the planetary effect on heredity appears twice as marked on a day of considerable disturbance in the solar field.

In the last 20 years, technical progress has made it possible to record more than merely the light coming from planets. Radio sources, gravitational

changes and magnetic variations have been recorded. Some of these could cause subtle hormonal changes, though it has yet to be shown how.

It is no longer impossible to imagine that scientists will find that man's biology and psyche have invisible connections with the clocks of the cosmos. And these connections may not be mysterious, astrological ones which would make puppets out of us—our fate controlled by external signs in the stars—but empirical ones, based on such forces as magnetism.

Greek philosophers noted long ago that man's character makes his destiny. The planets affect this, though they do not allow us to predict the future. And since the word *astrological* is associated with predictions and horoscopes, I think we should stop using the word and coin a new one once we have established the nature of the influence of the planets upon man's personality.

Astrology, Cosmobiology, and Humanistic Astrology

I. W. KELLY

Introduction

Astrology is a theory that alleges that there is a relationship between celestial events and the personalities and behaviors of human beings. Astrology has consistently been popular with the general public and, in fact, seems to be increasing in popularity. As Manolesco (1973, p. 91) pointed out:

> There are over 200,000 practicing astrologers in the USA and 20 astrological magazines, one of which prints half a million copies. Over 5,000 newspapers carry astrological forecasts. Five million Americans spend 200 million dollars annually, consulting popular astrologers.

In spite of public interest, astrology has generally been ignored by the scientific community. To a large extent this is the result of a rise in the number of alternative conceptions of human behavior in such fields as the social sciences, biology, and chronobiology (Kelly, 1979; Thagard, 1980*ab*), and because of an increase in astronomical knowledge that appears to be incompatible with many of the tenets of astrology (R. G. Culver and Ianna, 1979). Over the last few decades, however, several scientists have taken an interest in the claims of astrology and have tested many of its central tenets. The first section of this paper will outline these studies and describe the status of traditional astrology in the light of the scientific evidence.

The second section will describe the cosmobiological hypotheses of Michel Gauquelin, a French psychologist. He claims to have discovered a relationship between the location of various planets in the sky at the births of certain individuals in various occupations and their success in these occupations. The correlations reported by Gauquelin are of an extremely small

The author would like to acknowledge the comments of Rudi Krutzen on parts of an earlier draft of this paper.

magnitude but are statistically significant (that is, it is unlikely that they are due to chance).

The third section will describe another form of astrology that has gained popularity over the past decade: Humanistic astrology. Humanistic astrology purports to be a spiritual, philosophical enterprise rather than a scientific one.

A final section will briefly discuss the future of investigations into relationships between celestial and human behavior.

Traditional Astrology

Astrology claims to be able to describe and predict a person's behavior on the basis of a horoscope. An individual's horoscope is a geocentric map of the sky in which are located astrologically significant elements—such as zodiac signs, planets, the ascendant, and the houses—at the time of that individual's birth.

Astrologers visualize the universe as a giant celestial sphere with the earth as its center. From this perspective the sun and planets appear to move around the earth on a yearly basis. The sign of the zodiac one is assigned is determined by the time of year during which one is born. The sun appears to track through twelve astrologically significant constellations (Aries, Taurus, Gemini, Cancer, Leo, Virgo, Libra, Scorpio, Sagittarius, Capricorn, Aquarius, and Pisces) each year and spends time each year with one of these constellations in the background. The zodiacal sign one is born under is determined by the background constellation the sun is in when one is born. Each sign allegedly signifies different personality characteristics. For example, Libra, the Balance, purportedly indicates cooperation and harmony. A person born when the sun is in Libra should, theoretically, show a disposition to exhibit these traits. Astrologers contend that zodiac personality descriptions will apply substantially to those with the sun, moon, "ascendant," or the ruling planet in their respective zodiac sign at birth.

The signs of the zodiac can be grouped in various ways. Three traditional groupings of the signs are: (1) the polarities—the signs are divided alternately into positive and negative; (2) the quadruplicities—the signs are divided into cardinal, fixed, and mutable; (3) the triplicities—the signs are divided into the categories of air, fire, water, and earth. These groupings classify the signs according to various personality characteristics that are associated with each sign.

In addition to the sun's apparent annual motion around the earth, there is the daily rotation of the earth around its own axis. Just as astrologers divide the sky into twelve regions (the signs of the zodiac), so do they divide the sky into twelve sectors called houses, for the daily rotation. The sun and planets travel through these sectors on a daily basis. The houses are not given names

but are designated by numbers. Each house is associated with a particular aspect of life. The second house, for example, is associated with money and possessions. Of particular significance is the "ascendant," the zodiacal sign that is rising in the east when an individual is born.

The planets are also associated with specific influences on character. For example, Mars is attributed with combativeness and decisiveness. Each planet purportedly "rules" one or more of the signs. Whenever a planet is in the sign that it rules it has a strong influence on the individual born at that time. Pluto and Mars, for example, are the rulers of Scorpio. Angular relationships (aspects) between the sun and planets are also considered to be significant. Sextiles (60° angles) and trines (120° angles) denote harmonious personality functioning, whereas squares (90° arcs) and oppositions (180° arcs) represent inharmonious personality functioning.

The interpretation of the horoscope is governed by a consideration of the following elements: the particular locations of the planets in the signs and houses, the position of the signs with respect to the houses, and by certain angles determined by the planets' positions relative to each other.

There is some dispute in astrological circles regarding the resolution of the problem of the precession of the equinoxes. Because of the slow change in the earth's rotational axis over time, the signs of the zodiac and their namesake constellations no longer coincide as they did 2,000 years ago. Two contending zodiacs have arisen as solutions to the problem. The most popular zodiac, the tropical zodiac, is defined in terms of time of year; consequently, the actual positions of the sun and constellations relative to each other are ignored. On the other hand, a minority of astrologers adopt a sidereal zodiac and cast a horoscope on the basis of the constellations near the sun when one is born.

The most extensive outline and evaluation of studies of astrology is the recent compendium of G. Dean and Mather (1977). Dean and Mather outline the various systems of astrology and describe the scientific status of these systems. They coordinate and summarize material from over 700 astrology books and 300 scientific works, covering research conducted from 1900 to 1976. On the basis of the research examined, Dean and Mather contend that:

1. *Present-day astrological claims are based on opinion rather than on evidence*. The authors state (1977, p. 1) that:

> Astrology today is based on concepts of unknown origin but effectively deified as "tradition." Their application involves numerous systems, most of them disagreeing on fundamental issues, and all of them supported by anecdotal evidence of the most unreliable kind. In effect, astrology presents a dazzling and technically sound superstructure supported by unproven beliefs; it starts with fantasy and then proceeds entirely logically. Speculation is rife, as are a pro-

fusion of new factors (each more dramatically "valid" than the last) to be conveniently considered where they reinforce the case and ignored otherwise.

2. *The balance of evidence points to the rejection of signs, ascendants, houses, and planetary rulerships, the main tenets of traditional astrology.* Dean and Mather contend that on the basis of the behavioral evidence of studies of astrology, the balance of evidence is against the validity of zodiacal signs (1977, p. 75), houses (p. 165), and planetary rulerships (p. 203).

Dean (1979, p. 93; see also R. G. Culver and Ianna, 1979, pp. 139–151) also points out that it is difficult to believe that astrology is based on early observations of the heavens and behavior as many astrologers claim it to be:

> Mars rising at birth could not have been empirically related to achievement in sport, not because the ancients could not recognize sportsmen, but because the effect (if real) is small and cannot be detected without large samples and modern methods of analysis. Similarly, in traditional astrology the minimum package of 10 planets (including the moon) in 12 signs, 12 houses, and making 5 major aspects, provides nearly 500 different factors of which about 30–40 are present in the average chart. Hence to claim, as many astrologers do, that tradition is the result of millenia of empirical observation, is to claim that the meaning of each of 500 factors can be deduced when any 40 can be present at the same time. This is clearly untenable. Conversely, if the factors were so easy to observe, why is it that today there is not convincing evidence for any of them?

3. *Astrology can be largely explained by universal validity, bias in self-assessment, halo effects, and coincidence.* When astrologers are asked to produce evidence for their system, the most common reply is an "It works!" Astrologers appeal to the fact that horoscopes often match the personalities of their clients. As Dean and Mather (1977, p. 25) point out, however, "it has yet to be demonstrated that personality delineations using the right chart are significantly better than those using the wrong chart." The authors give a number of examples in which astrologers wrote horoscopes for famous individuals on the basis of the wrong chart but produced correct delineations nevertheless. Consider this example cited by Dean and Mather (1977, p. 29):

> Henderson presents a detailed astroanalysis of the character of Lenin and the events in his life. The birth data quoted are correct but the chart used corresponds to a date 12 days earlier; the angles are roughly correct but the signs, houses and aspects of the faster-moving planets are mostly incorrect. As a result, about half the significators are wrong yet they match the character and events perfectly.

Many psychological studies have demonstrated that people readily accept general statements as applying accurately to their unique personality (Dean and Mather, 1977, pp. 36–49; see also Tyson, 1979, pp. 202–211). For example, Snyder (1974) made a fictitious horoscope by taking general state-

ments from an astrology book and found that university students rated the horoscopes as being an above-average description of themselves.[1] Consequently, Dean and Mather (1979, p. 99) argue that most, if not all, of the apparent success of astrology "could be due to gullibility, universal validity, bias in self-assessment, halo effects, coincidence, and so on."

Several other studies have been conducted since the Dean and Mather book appeared that support their conclusions that there is little validity to traditional astrology (Kelly, 1979, 1979–1980). However, a study by Mayo, White, and Eysenck (1978) deserves special mention. They claimed to have demonstrated a relationship among introversion, extroversion, and the signs of the zodiac using the Eysenck Personality Inventory. The results of the Mayo-White-Eysenck study provided prima facie support for the two astrological hypotheses that subjects born under odd signs (Aries, Gemini, Leo, Libra, Sagittarius, Aquarius) show higher extroversion scores and that those born under the even-number zodiacal signs (Taurus, Cancer, Virgo, Scorpio, Capricorn, Pisces) show higher introversion scores.

Although the results were construed as supporting the astrological hypotheses, both psychology and chronobiology could produce alternative hypotheses for the data. Kelly (1979–1980) points out that an obvious alternative explanation would be a seasonal effect operating on a critical perinatal stage or phase. Wendt (1978) suggests a related explanation in terms of an imprinting or learning phenomenon that may be contingent on certain triphasic circannual functions. Another possibility is that very early critical stages (nearer conception perhaps) are also susceptible to some sort of electromagnetic field vector. Finally, one could consider the systematic self-attributions people make in light of their personal notions of "astrology." Pawlik and Buse (1979) followed up on the latter hypothesis and controlled for subjects' own beliefs of what their "zodiac signs" were supposed to mean. They found that the so-called "astrological effect," in the Hamburg students, could be statistically ascribed to their self-attributions. Subsequent research by Eysenck (1979) replicated the finding of Pawlik and Buse, and he concurred that the entire "astrological effect" was attributable to subjects' expectations and familiarity with "zodiac"-type assumptions. Eysenck tested over 1,000 children and they showed no sign of the effect. Nor did adults (Salvation Army students) when specifically selected to exclude any with knowledge of astrology. The effect appeared in the Mayo-White-Eysenck research because even the group classified as lacking knowledge probably knew about "zodiac signs" (and this, after all, is all that is necessary). They had in fact been classified on the basis of the question: "Do you know how to interpret an astrological chart?" which involves a relatively advanced level of knowledge.

The Mayo-White-Eysenck "astrological effect" is a very instructive example that underlines the need to eliminate all "normal" alternative

hypotheses before embracing a paranormal one. What at first looked to be supportive of a paranormal hypothesis was, with subsequent research, determined to have a normal explanation after all and indeed to count *against* the astrological hypothesis.

In addition, R. B. Culver (1981) found no relationship between zodiac sign and characteristics such as hair color, blood type, bust size, chest size, foot size and weight, and a variety of other physical characteristics. Gauquelin and Gauquelin (1978) examined two astrological hypotheses. The claim that individuals in various *occupations* tend to be associated with particular zodiacal signs, moon signs, planet signs, mid-heaven signs, and ascendant signs was tested by examining the distribution of these signs at the birth of 15,560 successful professionals belonging to ten different occupations. The claim that a zodiacal heredity effect exists (namely, that there is a tendency for children to be born under the same astrological factors as their parents) was tested by an examination of a total of 7,964 individuals and 3,923 parent–child pairs. In addition, three traditional groupings of the zodiacal signs were examined: polarities, triplicities, and quadruplicities. None of the astrological hypotheses under examination was supported. In addition, Tyson (1980) examined the relationship between date of birth and career, as defined by course of study, for 10,313 university graduates. The results indicated fairly conclusively that there was no relationship between birth date and career; hence it was concluded that neither astrological nor season-of-birth factors influence choice of career.

Snell, Dean, and Wakefield (1979) drew two random samples of 1,500 individuals each from leadership biographies to test the astrological claim that some zodiac signs should be overrepresented. No significant differences were noted among any of the signs. Rawlins (1979–1980) examined the positions in the sky of the sun, planets, and moon at the times of the birth of 407 prominent American athletes.[2] The areas of the sky examined corresponded to the twelve astrological houses. The results indicated no significant relationship between the time of an athlete's birth and the position in the sky of the sun, planets, or moon. Noblitt (1978) tested the astrological notion that the angular relationships (sextiles, trines, squares, and oppositions) among the celestial bodies at the time of a person's birth are predictive of that individual's personality traits. In addition, zodiac sign and angular relationships between the sun and the planets were tested for any possible relationship to personality traits. A sample of 155 volunteers were given the 16PF personality questionnaire and the above astrological factors were examined individually and in combination for possible correlations. No support was found for any of the astrological hypotheses tested. Danneel (1975) examined the relationship between zodiac sign and frequency of suicide in 592 cases of suicide in West Germany and found no relationship. Finally, Woodruff, Guze, and Clayton (1974) found no overall significant

relationship between zodiac sign and the individual diagnoses of a group of 497 psychiatric patients.

Astrologers tend conveniently to ignore these negative results or to dismiss them with the "all-factors-in-the-horoscope" argument. As Eysenck (1979, p. 87) puts it:

> What they would say is that you cannot separate out from the large body of astrological knowledge certain aspects and hope to get very positive results. Personality, they would say, is determined by so many things—aspects and positions of the planets, and of the moon, as well as of the sun, that any single element tends to be swamped by all the others.

This argument will not work, however. Studies of astrologers have been conducted in which significant factors were taken into consideration and, in general, the results have still failed to support astrology. Dean and Mather (1977) refer to studies examining the abilities of astrologers to distinguish between genuine and spurious horoscopes. Only a few studies have been conducted here and the results are mixed; some studies are supportive of the ability of astrologers to distinguish between correct and incorrect birth dates whereas others have produced negative results (Dean and Mather, 1977, p. 549). A well-designed set of studies that prima facie supports astrology was conducted by Clark in the 1950s (Dean and Mather, 1977, pp. 544–549). In the first study, astrologers had to match ten birth charts with ten individuals, given only their career descriptions. In the second study, astrologers had to match ten pairs of charts with ten case histories of individuals. One of the pair was fabricated. Finally, astrologers had to distinguish correctly between ten pairs of birth charts, in which one of the pair belonged to a person with high intelligence and the other to a person with severe brain damage. Out of a total of 200 judgments, astrologers were correct 64 percent of the time, whereas 50 percent would be expected by chance. Hence, although the results are statistically significant, they are of only marginal usefulness.

A large number of studies suggest that astrologers, with significant factors taken into consideration in the horoscope, have produced results not supportive of astrological claims. Astrologers have, in these studies, revealed no ability to distinguish between gifted and retarded individuals, suicidal and non-suicidal individuals, or alcoholic and non-alcoholic individuals on the basis of birth information (Dean and Mather, 1977, p. 549; Macharg, 1975). Gauquelin (1970) reports on an experiment in which fourteen astrologers were given the names and birth information of three celebrities—a television announcer, a bicycle-racing champion, and an entertainer. The astrologers were required to match the names with the birth information on the basis of their interpretations of the horoscopes constructed on the birth information given. No astrologer did better than would be expected by chance. Another

study by Gauquelin and Gauquelin (1981), one using the "character-traits" method, also failed to support astrology. They gathered 60,000 trait units from 2,000 biographies and tested the validity of the psychological descriptions of the twelve zodiacal signs. The Gauquelins noted all the character traits considered typical by internationally known astrologers (J. Mayo, A. Barbault, P. Grell, M. Hall, C. O. Carter, M. Hone, D. Lee, and A. T. Mann) for all the signs. The claims of these astrologers were examined individually. The positions of the sun, the moon, the ascendant, and the planets at the births of the 2,000 people having an Aries trait, Taurus trait, and so on, were calculated. But the persons described by Aries traits, for example, in the works of the astrologers were not born more often with the sun, the moon, or the ascendant in Aries. The Gauquelins also tested the sidereal zodiac with negative results, thus supporting the earlier study by Hume and Goldstein (1977) who also failed to obtain results supportive of either the tropical or sidereal zodiacs.

Fourie, Coetzee and Costello (1980) tested the hypothesis that the astrological chart is more effective in predicting personality than the zodiac sign alone by giving two prominent astrologers the sex, birth place and times of birth of 16 men and 34 women. There was not only little agreement between the two astrologers but neither of the astrologers could predict the subjects' personality traits as measured by the 16PF personality test.

Tyson (1979), in South Africa, had a prominent professional astrologer cast horoscopes for fifteen individuals given the time, date, and place of their birth. The astrologer, the subject, and a person who knew the subject very well were asked to rate the subject on five personality dimensions (social unresponsiveness vs. social responsiveness, tranquillity vs. anxiety, amity vs. hostility, flexibility vs. rigidity, and submissiveness vs. dominance). The subjects were also required to fill out the South African Personality Questionnaire which measures these dimensions. The horoscope of each subject was then combined with four other randomly selected horoscopes to form a set of five horoscopes. All subjects were given this set of horoscopes and asked to indicate which of the five they believed to be their own. The same sets of horoscopes were given to the nominees, with the same instructions. The nominees were asked to do the task independently and not to discuss it with anyone until they had completed it. The subjects and nominees were unable to identify the subjects' horoscopes at a better than chance level. The intercorrelations between the personality test scores, self-ratings, ratings by nominees, and ratings by the astrologer on each of the five scales also proved interesting. The astrologer proved unable to predict either the test scores, the subject's self-ratings, or the ratings of the subject by the nominee. In fact, many of the astrologer's predictions were the reverse of what was really the case. On the other hand, both the subjects' and nominees' ratings correlated in most cases with the test scores, and there was a significant

relationship on four of the five personality dimensions between the subjects' self-ratings and the nominees' ratings of the subjects.

Culver and Ianna (1979) examined a total of 3,011 predictions comprised of those made by leading astrologers (Carroll Righter, Sydney Omarr, etc.) over a six-year period and those recorded in astrological publications in the same period. Only eleven percent of the predictions were correct: no astrologer was able to predict correctly more than twenty percent of the time, and most had considerably smaller rates of success. Moreover, correct predictions were most often those that could be attributed to shrewd guesses (the SALT talks will continue to be stalled for another year), vague wording (there will be a tragedy in the eastern United States during the spring months), and inside information about the individuals involved (Starlet A will be married to Director B before Christmas). In addition, Hunter and Derr (1978) checked astrologers' predictions regarding the location, time of year, and size of earthquakes. The astrologers scored significantly worse than chance.

On the basis of existing studies one can conclude that the evidence is against the validity of astrological tenets. Studies of astrology tend to fail to support claims regarding astrological factors whether they are considered in isolation or in combination.

The Cosmobiology of Michel Gauquelin

Michel and Francoise Gauquelin[3] (1977–1978) have been examining astrology and possible relationships between planetary positions and occupation and temperament for the last two decades. They have utilized archival data on more than 40,000 individuals in Europe. Since the births listed in the countries examined include both date and hour of birth of each individual, one can work out the daily positions of the sun, moon, and planets at each person's birth. The individuals examined by the Gauquelins come from ten selected occupations (actors, artists, businessmen, journalists, physicians, politicians, scientists, soldiers, sports champions, and writers). They were also able to obtain biographical information on each individual through biographical dictionaries and canvassed opinions. They have been able to divide individuals in each occupation into two groups—eminent and non-eminent. The criteria chosen for this grouping have apparently been quite objective: a person was considered eminent, for example, if he was given a biographical description in one of a number of periodicals (for example, being mentioned in the *Dictionnaire des Sports*).

The Gauquelins have also checked the biographies of the individuals in the various occupations and for each person words relating to personality characteristics have been collected and analyzed. From an analysis of the personality descriptions given in the biographies they derived a "modal" personality for each occupational group. The "modal" personality tended to

describe most individuals within each group. Hence the Gauquelins have established an ideal context under which to test for possible relationships between celestial configurations and human affairs. On the basis of their extensive research they claim to have discovered some interesting correlations between planetary positions at the births of various individuals and occupational and personality variables. The results of their research can be outlined as follows.

1. *There is a relationship between planetary position in particular sectors of the sky at the birth of various individuals and their occupations and personality.* The apparent daily motion of a planet around the earth can be divided into twelve sectors (Figure 1). One would expect there to be an equal probability for individuals to be born in any of the twelve sectors. That is, one would expect roughly one-twelfth of individuals in either the general population or in the various occupations to be born in each sector. This is not the case, however; the frequency of birth of individuals in the general population and within each occupation (both eminent and non-eminent) is at a maximum in the morning and at a minimum in the afternoon. Above and beyond this, Gauquelin (1974, 1980*b*) claims to have found correlations between the position of certain planets (Venus, Mars, Jupiter, and Saturn) and the moon and the birthrate of successful individuals in various professions. He claims to have found a nonrandom relationship between these births and planets in four key sectors: 1, 4, 7, and 10 (Figure 1). The strongest relationship is found with sectors 1 and 4. For example, notable athletes tend to be born with Mars in sectors 1 and 4 more often than would be expected by chance; famous physicians and scientists tend to show this relationship with Saturn, and so on.

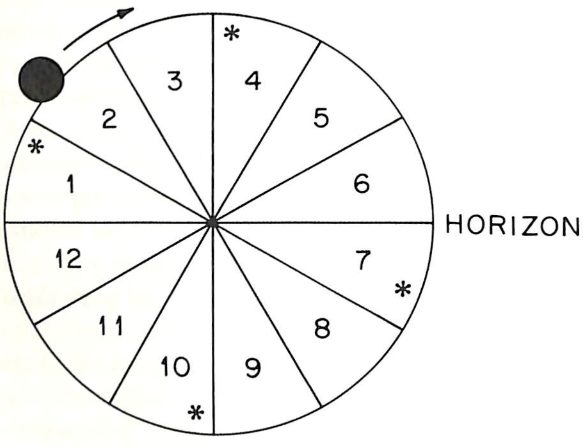


Figure 1. The apparent daily motion of a planet around the earth divided into twelve sectors. Asterisks mark the four significant sectors for Gauquelin.

It is important to emphasize that the relationship between planetary position at birth and occupation can only be found with the most eminent at each profession. Less famous individuals in the same profession show no such persistent relationship with planetary position at birth.

Since the planetary relationship can only be found with a proportion of the most noteworthy individuals in each profession, Gauquelin argues that personality variables are responsible for the relationship rather than occupation variables. He found, for example, that individuals exhibiting the modal personality for each occupation showed the strongest relationship with planetary configuration. Individuals, irrespective of occupation, who showed the modal personality of, for instance, the scientific group tended to have the same planetary pattern present at their birth as the scientific group (Gauquelin, 1980*b*; see also Gauquelin, Gauquelin, and Eysenck, 1979). The relationship between personality and planetary position, then, is stronger than that between occupation and planetary position at birth.

Gauquelin (1974) also claims to have uncovered a "planetary heredity." He argues that since personality traits are largely inherited, any relationship between planetary position and personality should also be detectable in the children of individuals who exhibit the significant planetary configuration at birth. He examined the hour and date of birth of 25,000 parents and their children and found a tendency for the parents and children to have the same planet(s) in the significant sectors at their births. In addition, the relationship was twice as strong if both parents had the same planet in the significant zones at their birth. Finally, siblings were found to have similar planetary patterns more frequently than unrelated children (Gauquelin, 1960).

2. *The planetary relationships are extremely weak and growing weaker with the advent of non-naturalistic birth delivery methods.* Gauquelin has pointed out that the purported planetary effect on professionals only occurs under two conditions, namely, the births are natural and the professionals are very famous individuals. The intensity of the planetary effect is strongly related, he claims, to the degree of fame and achievement of the individuals involved.

Regarding the first point, he points out that there is a tendency toward medical intervention in recent births and that this intervention makes the effect more difficult to detect:

> As the "natural" diurnal rhythm of delivery is disappearing, so too is the planetary effect. Modern medical practices are replacing the cosmic environment and masking its role: . . . today there are many more new obstetrical techniques. The very onset of labor is often induced artifically for reasons of convenience. A study of the planetary effect on births today would necessitate a concerted effort to separate physiological birth from birth induced by medical means. (Gauquelin, 1974, p. 202)

Gauquelin (1960) also argues that the "planetary effect" is detectable only when the "all-time greats" in an occupation are included in the sample.

Consider the "Mars effect" and sports champions: he contends that even all-star athletes may not show the Mars effect:

> What makes [moderately famous athletes] different from the famous champions is their incapacity to pull themselves up to the top rank in very important competitions. Our investigations of the personality of athletes offer an explanation of this phenomenon: the relationship between Mars and success in sports is weaker than the correlation between Mars and the temperament. Highly successful champions very often possess what we describe as the "Mars temperament." Such temperament is not absent in less renowned athletes, but it is less marked and not more frequent than in nonathletic people. (Gauquelin and Gauquelin, 1979–1980, p. 39)

3. *The planetary relationships are explained in terms of a "cosmic genetics."* Gauquelin denies that the planets directly influence the personality of individuals at or before birth. Instead, he tells us:

> The child reacts to the passing of the planet which is simply evidence of his temperament—simply evidence. It in no way modifies the child's future personality at birth and does not give him anything not already in his genetic constitution. (Gauquelin, 1974, p. 203)

Gauquelin's (1974) explanation involves a set of speculative links: certain planetary configurations tend to affect certain changes in solar activity which in turn affect geomagnetic or electromagnetic fields at the earth's surface which tend to initiate labor in fetuses with a particular genetic structure. Since personality is largely inherited, he argues that individuals with such a genetic structure will tend to grow up to be individuals with a particular temperament. And successful people in particular occupations, such as the ones he has studied, usually have a modal personality. Individuals with a genetic structure for such a personality are sensitized to particular changes at the earth's surface caused by a particular planet(s) rather as individuals of different skin colors react differently to solar radiation.

4. *Traditional astrology is rejected.* The archival material collected by Gauquelin has proved convenient for the testing of traditional astrological claims. He has tested astrological hypotheses extensively with both the occupational data and the temperament information (Gauquelin, 1974, 1980*a*). The results have all been negative, whether all the subjects were examined together or whether they were divided into categories of eminent and non-eminent. Since very large numbers of individuals have been used in Gauquelin's tests of astrological influence one would expect any relationship to show up, even if it was extremely weak.

Gauquelin (1974, 1980*a*) has determined that the signs of the zodiac, the houses, and the aspects of the planets relative to each other at the time of a subject's birth have no relationship to an individual's heredity, choice of

occupation, or temperament. He also claims to have shown the fallacy of the horoscope (Gauquelin, 1968). Consequently, he argues:

> Every attempt, whether of astrologers or scientists, to produce evidence of astrological laws has been in vain. It is now quite certain that the signs in the sky which presided over our births have no power whatever to decide our fates, to affect our heredity, characteristics, or to play any part however humble in the totality of effects, random and otherwise, which form the fabric of our lives and mold our impulses to action. (Gauquelin, 1969, p. 145)

Interestingly, however, the relationships claimed by the Gauquelins to exist between the occupations and temperaments and particular planets are, in many cases, but not all, those one would expect on the basis of traditional astrological tenets (Dean and Mather, 1977, p. 216; F. Gauquelin, 1980). For example, Mars is the symbol of strength, violence, and military matters and is indeed a planet that tends to occur in the key sectors at the births of eminent military men. Gauquelin, however, considers these correspondences with traditional astrological lore to be just a fortuitous coincidence. He points out that:

> The symbolism of the other planets in the solar system is not demonstrated by statistics. The sun does not appear among the leaders (military, political, etc.); nor Venus for artists (musicians, painters, actors); nor Mercury for writers and businessmen. . . . And none of the more distant planets—Uranus, Neptune, and Pluto—justifies the symbolism that modern astrologers have lightly attributed to them. (Gauquelin, 1979*a*, p. 153)

Replications of Gauquelin's Work

Although the magnitude of the relationship between planetary configuration and temperament claimed by Gauquelin is very small (even within a group of individuals with a particular modal personality the majority have a random distribution of planets at their birth) it is statistically significant, and if his claims are valid they may herald a new science. Hence, the attempted replications of Gauquelin's claims that have been carried out are of great importance.[4] All these studies have involved the alleged Mars effect with athletes.

The earliest attempted replication was conducted by the Belgian Comité Para (Comité Para, 1976), a Belgian committee for the scientific examination of claims of the paranormal. The committee repeated Gauquelin's procedure with an independent sample of 535 sports champions and obtained prima facie results supportive of Gauquelin's hypothesis: sports champions were born significantly more often with Mars in sectors 1 and 4 than were un-noted persons. However, Rawlins (1978) and Kurtz, Zelen, and Abell (1980) have pointed out that other sectors, as well as 1 and 4, had unexpectedly higher frequencies. For example, sectors 9 and 10 had higher frequencies than the

key sector 4, and sector 8 also had a higher frequency than would be expected by chance. Hence the Comité Para data provide only partial support for Gauquelin's hypothesis, in marked contrast to Gauquelin's claims of complete support.

A more controversial experiment has been the recent replication attempt using data obtained from a group of 407 prominent American athletes. Analyses of the data by Rawlins (1979–1980) and Kurtz, Zelen, and Abell (1979–1980*a*, 1980) suggest that the data do not support the hypothesis of the Mars effect with sports champions. The Gauquelins (1979–1980; M. Gauquelin, 1980*b*) have responded that although the athletes examined by Rawlins and Kurtz, Zelen, and Abell are indeed well-known they are not "the greatest names in American sports." They then selected from the sample the 88 most famous athletes and found the Mars effect with these. Kurtz, Zelen, and Abell (1979–1980*b*, 1980) pointed out in their replies that Gauquelin has been shifting his criteria for what constitutes an eminent athlete. They argue that the criteria used for his analysis of the American sample are far more restrictive than those used in the samples that originally suggested the Mars effect. They argue as well that, as in the Comité Para replication, other sectors appear as prominent as Gauquelin's key sectors. For example, contrary to what would be predicted from Gauquelin's hypothesis, Mars appears in sectors 7 and 8 more often than in sector 4, and as often in sector 10 as in sector 4. Gauquelin could respond, of course, that these unpredictable occurrences are simply aberrations, but this would seem to be both ad hoc and self-indulgent.

Gauquelin (1979*b*) also produced another replication using 432 European athletes. His analysis of the data indicates support for the Mars effect in very famous athletes, but it could not be detected with less prominent sportsmen. Kurtz, Zelen, and Abell (1979–1980*ab*, 1980) have commented on this new experiment by arguing that the criteria used by Gauquelin have shifted from those he used in other studies. By using criteria not unlike those employed by Gauquelin in his recent European replication, they claim to show that alternative subsets of equally prominent athletes do not show the Mars effect. It would appear then that there are several factors other than a true Mars effect that might account for the results.

Over the past few decades another form of astrology has grown popular at the expense of empirical, traditional astrology, one that ties in its tenets with those of the now popular transpersonal psychologies and mysticisms of the East. It is to this form of astrology we will now turn.

Humanistic Astrology

In this section I will critically examine the main tenets of what is called Humanistic astrology. Although there are many forms of Humanistic astrol-

ogy, there are claims common to the various positions. My main concern here will be to outline the main tenets of the position of Humanistic astrology's leading advocate, Dane Rudhyar.

Rudhyar attempts to relate astrology to holistic eastern thought. In contrast to traditional astrologers who view astrology as a science, the Humanistic astrologer views astrology as a philosophy of life. Rudhyar puts forward the following claims for his brand of astrology.

1. *Astrology, unlike science, deals with the uniqueness of individuals.* Rudhyar (1936/1970, p. 460) informs us that "modern science is obliged to ignore the individualness of every living entity" whereas astrology "deals essentially with the individual."

Science does aim at the general in the form of laws, but it does not follow that it is indifferent to individuals. The search for general laws is conducted in order to understand the individual members of a class of entities or events. Indeed, an ultimate test for a scientific theory is whether or not it explains individual events. For example, a test of a theory of learning in psychology is whether or not Johnny will learn better under the conditions specified by the theory. If the theory does not explain children's behaviors then we reject or revise it.

Every person is unique in that he has his own personality and physical characteristics, but he is not unique in that he has many characteristics (chemical, psychological, and biological) that he shares with other human beings. Because of these commonalities we can understand specific interactions between individuals. No doubt our theories in psychology and the social sciences are not as complete as those in the physical sciences, but we have made large progress over the past fifty years, progress that has contributed to our understanding of individual human beings.

And if the Humanistic astrologer is interested in individuality and uniqueness, why is he not more interested in autobiographies, art, literature, private journals, and so on? Over the past centuries, literature and art have certainly contributed more to society than astrology.

2. *Astrology is not concerned with precise predictions and precise character analysis.* "Traditional astrology claims to be able to provide an accurate character analysis of an individual and, under certain conditions, to provide precise predictions regarding that individual's behavior." Rudhyar eschews such an approach. He rejects traditional astrology as outmoded and confused. He tells us that he sees

> no value in the prediction of exact events or even of precise character analysis. Since I started writing on astrology in 1933, I have received many letters from people telling me how fearful or psychologically confused they had become after consulting with a well-known astrologer and being given biased character analyses and/or predictions of illness, catastrophe, or even death. Statistical research may help to show how unscientific such predictions usually are, but to

> me what is most necessary is to give to the question I have consistently asked, "What is astrology FOR?"—a more basic, philosophical and spiritual answer. (Rudhyar, 1979, p. 85)

According to Rudhyar, the birth chart or horoscope of an individual's birth, when interpreted, helps one better to understand that individual's innate potentialities. The planetary positions at the time of an individual's birth reveal the set of potentialities imprinted in that person's genetic code at birth but do not tell us what precise events or relationships will actualize these potentialities (Rudhyar, 1975*a*, 1977). How, then, does Rudhyar know that the potentialities delineated in the horoscope are the correct ones for the individual? On the other hand, if questions of validity are irrelevant, why should anyone master Rudhyar's system and go to so much trouble to erect a horoscope for people? Why not just give everyone the same horoscope or variations on a recurrent theme?

3. *Astrology's truths are not empirical.* Rudhyar (1979, p. 84) tells us that he is "only interested in astrology as a means to help human beings give a fuller, richer meaning to their lives and to their relation to the universe in which they live." He denies that astrology gives empirical truths; instead it is sufficient if "after having studied . . . his . . . birthchart, [an individual] is able to feel a direction and purpose in his life" (Rudhyar, 1936/1970, p. 7). What about an individual who has a more pronounced sense of direction when he believes he is Napoleon and that the psychiatric hospital attendants are the Duke of Wellington and the czar of Russia and are his servants? Would Rudhyar advocate that this individual continue to believe that he is Napoleon? Even if a person feels more self-satisfied as a result of a particular set of beliefs, that individual may still be mistaken in his beliefs: he may have been misinformed, for example. The problem with all of Rudhyar's writings is that his way of putting things is obscure to say the least, and perhaps deliberately obfuscatory as well. What, for example, is the meaning of a "full life" or "richer meaning to life"? Would more money, becoming taller, changing one's appearance, or becoming more self-satisfied contribute toward a fuller or richer life? Consider, also, the following from Rudhyar (1975*b*, p. 24):

> The essential factor in the transformation of man's consciousness is the transmutation of the "solar" I into the "galactic" We. In this We-consciousness the principle of interpenetration operates. This is the galactic dimension of existence. In it, the sense of the separateness of isolated (i-so-lated!) entities (which are strictly and exclusively what they are) vanishes. Everything not only is related to everything else, but, I repeat, every entity—every mind, too—interpenetrates every other entity. As the consciousness of an individual person is able to operate in this spiritual dimension, it begins actively and transformatively to participate in the process of integration of humanity at the level at which the formation of a "pleroma" (or fullness) of Man is possible—the level

> of the spiritual mind or supermind. At that level unanimity in consciousness prevails, yet each participant in the pleroma—or as a true occultist would say in the "White Lodge"—retains the ability to operate.

What is the status of these remarks? Could anything falsify these claims? If not, what is being claimed? The statements do seem to be meaningful: for example, we have some idea of an entity "interpenetrating another entity," but just what is meant here? Does "interpenetrate" mean "have an effect on"? Consider, also, the statement "everything is related to everything else." Does Rudhyar mean that everything is causally related to everything else? Or is everything related in the sense that everything in the universe has a common cause? Perhaps the statement is trivially true. One can always find a relationship between two or more things—even if it is just the relationship of not being the same thing as the other thing (the relationship of being different).

Ironically, the symbolism chosen by Rudhyar to convey meaning is so obscure that one wonders how it could give richer meaning to anyone's life. Surely it is odd to go to so much trouble to express values and purposes in such a form.

4. *Astrology adopts a holistic view of man and the universe.* Rudhyar (1979, p. 84) contrasts his own approach to that of the materialists: "If one accepts the philosophy of Cartesian mechanism or 19th century 'scientific' materialism and the concept of every human being as a separate, essentially self-determined atomistic kind of individual, then man is understood in a certain way," whereas his own view is a "Holistic approach which is the foundation of the most recent physical theories in which the universe is shown to be a web of relationships, and all separate objects—including even atomic particles—are taken to be nothing but particular relationships" (Rudhyar, 1979, p. 83). There is a great difference between Rudhyar's astrology and modern physics, however. The recent physical theories have been arrived at through the testing of previous theories that have been rejected because they could not account for certain recalcitrant phenomena. Quantum mechanics, for example, is a physical theory developed to account for such phenomena. Rudhyar (1979, p. 84) tells us that "the symbolism of astrology can be very useful—somewhat as the symbolism of group algebra has proven useful—in interpreting the tracks produced by nuclear particles when atomic nuclei are violently broken down." But in what sense is astrology useful? We can test the usefulness of group algebra: it has a predictive value. What would be the equivalent test of the symbolism of astrology?

One is left with an unsettled state of mind concerning Humanistic astrology. Why does Rudhyar think any part of his astrology is true? Does he have an extra faculty by means of which he knows this? Or did he have experiences in which he acquired his concepts? If so, although one may not deny that they

have a significant value for Rudhyar, how does he know that such experiences are not merely subjective ones that do not reveal any reality beyond the experience itself? (See Carloye [1980] on this point.) One can also question whether it is in fact psychologically true that clients of Humanistic asrologers are made to be more contented or to lead fuller lives by following their precepts rather than those offered by alternative positions such as religion or one of the secular humanisms.

Conclusion

Three approaches to relationships between celestial and human affairs have been examined in this paper.

The first area covered was traditional astrology.[5,6] On the basis of the evidence, it would appear that there is negligible validity to traditional astrology. The only strong evidence for traditional astrological tenets is the weak relationships found in Gauquelin's research among Mars, Jupiter, and Saturn and a variety of careers and temperaments that are astrologically symbolized by these planets. It would appear, however, that these correspondences with traditional astrology are, as Gauquelin claims (1979*a*), merely coincidental. First of all, the astrological symbolism of the other planets is not, in general, borne out by research. Second, the purported relationships uncovered by Gauquelin are far weaker than traditional astrology would suggest. Instead of correlations with individuals in general in a particular career, eminent or non-eminent, his research suggests that only *some* of the most historically outstanding individuals in a particular field have the requisite planets in the key zones at their birth. Since the effect does not exist with the rest of us, it consequently has no relevance to our horoscopes. Third, the relationship between planetary position and career and temperament cannot be detected without sophisticated statistical tools and hence could not have formed the basis of ancient tenets of astrology (Abell, 1979, p. 77; G. Dean, 1979, p. 93). Fourth, as the astrologer Malcolm Dean (1980, p. 238) points out, the predictions of the Gauquelins and those of the astrologers are of a different nature:

> The essential difference between what Gauquelin discovered and what astrologers study is that Gauquelin is speaking of statistical probabilities in his predictions or character analyses. For most astrologers, the world operates on the principle of synchronicity, not chance. . . . That is, everything which occurs at a given moment is connected through the various qualities of consciousness, time, and space at that instant. (p. 237; see also pp. 60–61)

This is indeed a big difference! And the Gauquelins are interested in providing *physical* explanations of their correlations, not astrological or occult explanations. Consequently, Gauquelin does not consider his research to be astrology, but the new science of cosmobiology.

However, questions remain regarding the validity of Gauquelin's hypotheses. Both theoretical and empirical problems have been noted. Krips (1979) has pointed out that the explanatory mechanisms postulated by Gauquelin to account for his correlations could not account for them. He contends that the explanations Gauquelin proposes are inadequate in that they depend either on heliocentric aspects (which are at variance with Gauquelin's own statistical data) or on the empirically problematic notion that planetary wakes in the solar wind (operating in a "downward" fashion!) affect the solar radiation field at the earth's surface. In addition, Krips points out that Gauquelin's explanatory program rests on the controversial assumption that those particular personality dispositions that promote success in a certain profession have a large genetic component. This lowers the credibility of his hypotheses somewhat. In addition to these theoretical problems, Rawlins (1978, 1979–1980) and Kurtz, Zelen, and Abell (1979–1980*ab*, 1980) have pointed out other problems in the research, namely, the fact that other sectors than Gauquelin's favored ones show above-chance frequencies, contrary to what would be expected on Gauquelin's hypotheses, and the possibility that the criterion of professional eminence differs across studies (see also Gunter, 1980).

On the other hand, Gauquelin's claims cannot be dismissed with a clear conscience. He and his collaborators have amassed a large amount of evidence that prima facie supports his hypotheses. Further research is required before one can reach firm conclusions. One possibility might be to examine the planetary configurations at the births of all Olympic gold medalists in Asia and Australia. As a control one could examine the planetary configurations of other individuals from the same areas that have participated in the Olympic games without such success. Since only the most outstanding athletes (by anyone's criterion) would be included, the Mars effect should be evident if Gauquelin's hypotheses are valid. Another possibility is to examine the hypotheses regarding temperament and planetary position rather than occupation and planetary position. Since the relationship between planetary position and temperament is, as Gauquelin and Gauquelin (1979–1980) claim, a stronger one than that between occupation and the planets it would be feasible to give personality tests to large groups of individuals and determine if those individuals who exhibit, for example, a "Mars temperament" in fact have Mars in the crucial zones more often than those who do not exhibit such a temperament.

A third area examined was Humanistic astrology. This kind of astrology is viewed by its advocates as a philosophical, spiritual enterprise rather than as a science. Apart from the extreme vagueness of its precepts, one wonders what Humanistic astrology can provide that other philosophical or religious approaches cannot provide.

Notes

1. In addition to the use of general statements in horoscopes, an astrologer can pick up cues to the individual's personality, educational level, and so on from the appearance and verbal behavior of the client (Hyman, 1977). Tyson (1979) also found that consultation of an astrologer by a client is usually a response to stresses generated by an individual's social roles and inability to relate well to others. The astrologer could make use of facts such as this in formulating the horoscope.

2. This study was conducted in order to test the cosmobiological claims of Gauquelin. Whereas traditional astrology makes claims about the positions of various planets at the birth of athletes *in general*, Gauquelin makes claims about positions of planets at the births of historically outstanding athletes only.

3. Michel Gauquelin has been the main researcher, although his wife has collaborated on a number of studies.

4. Gauquelin himself conducted several successful replications after the phenomenon first suggested itself. However, extraordinary results should be replicated by independent investigators before they can be seriously considered.

5. A related popular set of beliefs relates phases of the moon to human activities. Over forty studies have been conducted to examine such claims. No support exists for belief in a relationship between moon phase and human behavior (Abell, 1981; Campbell and Beets, 1978).

6. There are also many theoretical problems with astrology. These are covered in R. G. Culver and Ianna (1979) and Kelly (1980).

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Suggested Readings

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SECTION II

SCIENCE OR PSEUDOSCIENCE?

THE PROBLEM OF DEMARCATION

Introduction

Parapsychologists are quite unanimous in upholding their discipline as a science. Advocates of the existence of flying saucers often present studies that aim at being scientific, and many characterize themselves as "UFOlogists" in that spirit. Some astrologers, though by no means all, think of astrology as a science.

Those antagonistic to these pursuits routinely condemn them as "mere pseudoscience." The critics' use of the term "pseudoscience" suggests not that parapsychology, astrology, and "UFOlogy" are merely unpopular science, or even bad science, but that they are something quite different; something that does not qualify as science at all, popular or unpopular, good or bad. Those who reject these as "pseudoscience" hold them to be *un*-scientific pursuits; poor facsimiles or corrupt imitations of science proper. Fool's gold–iron pyrite–is not gold at all. On this analogy, "pseudoscience" is intended to designate something like "fool's science."

This immediately raises a fundamental question in philosophy of science: What is science and what is pseudoscience and what is the essential difference between them? How are we to test astronomy and astrology, parapsychology and psychology, or particular claims or theories or modes of procedure in these areas in order to tell whether they qualify as scientific or not? In general, philosophers have not wanted to say that those areas of inquiry commonly labeled "pseudoscientific" are merely unpopular areas of investigation, or that the line between "science" and "pseudoscience" is a line drawn for a variety of historical, sociological, and broadly political reasons but without any firm philosophical justification. Surely science and pseudoscience must be fundamentally different–or so it seems. But where precisely does the difference lie? This, following Karl Popper, is what has been termed the "problem of demarcation." It is this question that we first raised in discussing the case of astrology, which we will address in general terms in this section,

and which will linger in one form or another throughout the volume.

CLARK GLYMOUR and DOUGLAS STALKER start off this section with a humorous piece satirizing the popular and lucrative forms of pseudoscience they see all around us. They suggest six dubious principles for constructing one's own pseudoscience, present their own bogus "Alphabetology" and "Fascinating Rhythms" in some detail as illustrations, and finish by offering the reader a franchise in a questionable enterprise called "Peruvian Pick-Up Sticks." This entertaining piece does not pretend to be a serious philosophical treatment of demarcation. But it does quite effectively raise the issue.

The next selection is KARL POPPER'S classic formulation of the problem of demarcation. Popper asks, "When should a theory be ranked as scientific?" and distinguishes this question from "When is a theory true?" or "When is a theory acceptable?" Popper rejects the suggestions that those theories are scientific that appeal to empirical evidence or that are sufficiently verified or confirmed. Instead he proposes a number of requirements that can be summarized by saying that "the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability." For Popper a theory is scientific only if it puts itself at empirical risk, and confirming evidence counts for a theory only if it results from an attempt to refute the theory. A theory that is not refutable by any conceivable event is non-scientific. On the basis of this falsifiability criterion of demarcation Popper condemns not only astrology but also the Marxist theory of history as well as Freudian and Adlerian psychology as pseudoscientific.

DANIEL ROTHBART argues that Popper's solution to the problem of demarcation is too weak; that a criterion of falsifiability fails to exclude too many clear cases of pseudoscience. On the basis of a discussion of criteria for criteria of demarcation that is intriguing in its own right, Rothbart proposes an alternative to Popper's criterion that is more historically oriented. Whether a theory is to be considered scientific or not, Rothbart maintains, may depend on what other theories it has as competitors at a particular time. In order to be scientific, he proposes, a theory must match the empirical and explanatory success of its rivals and must clash with its rivals by yielding inconsistent test implications. On this criterion astrology may have been a science in the context of ancient Greece, though it is not a science now because its competitors have changed so drastically. In his final section Rothbart pursues some of the implications of this approach and attempts to counter objections.

A more familiar historical approach is that of THOMAS S. KUHN. Kuhn here accuses Popper of taking as characteristic of science certain instances of scientific revolution that are in fact extraordinary and exceptional in the history of science. "Normal science" consists not of dramatic attempts to falsify major theories but of problem-solving within a context of assumed

and unquestioned theory. What is standardly tested in normal science is not those theories assumed as background but the competence of the individual investigator. For Kuhn it is normal science that holds the most promise of distinguishing science from other pursuits, and thus "if a demarcation criterion exists . . . it may lie just in that part of science which Sir Karl ignores." Kuhn agrees with Popper that astrology is an instance of pseudoscience, but for quite different reasons. Astrology does not fail to qualify as a science for lack of falsifiability; the history of astrology "records many predictions that categorically failed." Astrology fails to qualify as a science, Kuhn maintains, because it lacks the puzzles and problem-solving characteristics of normal science, and thus "could not have become a science even if the stars had, in fact, controlled human destiny."

In "Kuhn, Popper, and the Normative Problem of Demarcation," ROBERT FELEPPA attempts to evaluate the debate between Popper and Kuhn and to come to some resolution. Feleppa criticizes Kuhn's treatment of Popper in the preceding selection both on specific points of history and interpretation and on the more general grounds that Kuhn has missed the major point of Popper's work. Popper's attempt is not merely to draw a line between science and pseudoscience that is historically accurate as a description, but to *justify* the praise of some things as science and the condemnation of others as pseudoscience. Feleppa also offers a "reconstruction" of Kuhn's work, however, which seems better equipped than Kuhn's original to handle the tasks of this normative problem of demarcation. Here Feleppa introduces a pragmatic notion of justification involving "reflective equilibrium," borrowing from the work of Carl Hempel, John Rawls, and Nelson Goodman. A similar approach to Popper's work, he suggests, allows us to see the relative strengths and weaknesses of a Kuhnian and Popperian approach to the normative problem.

In the final piece of this section, ROGER COOTER presents a quite different perspective on science and pseudoscience. Cooter presents in some detail the social history of nineteenth-century phrenology as an illustration of his more general claim that the use of the term "pseudoscience" has played an "ideologically conservative and morally prescriptive social role" in the interests of an established capitalist order. Here Cooter explicitly adopts a relativist position concerning scientific truth, viewing those things that have been labeled "science" and "pseudoscience" as competing social institutions rather than as "correct science" and "false science." In the end Cooter proposes that the term "pseudoscience" be replaced by "unorthodox science" or "non-establishment science" so as to more adequately represent "relative differences in scientific social perception rather than categorical methodological opposites or matters of truth and falsehood."

Cooter's closing piece is clearly in conflict with much that precedes it. The general attitude he conveys is diametrically opposed to that evident in

Glymour and Stalker's opening piece, and his adoption of a relativist position clearly amounts to an abandonment of the normative problem of demarcation as presented by Popper and as discussed by Feleppa. Is Cooter's relativism an acceptable position? Does Cooter argue effectively for an abandonment of the attempt to construct and defend a viable criterion of demarcation?

Consider also a particular bone of contention that is bound to arise between Popper and Cooter. Cooter's is a historical argument, in the general tradition of Marxism, for abandoning any attempt at demarcation in Popper's sense. But one of the things that Popper explicitly rejects as pseudoscience is Marxist theory of history.

The selections of this section as a whole, and the disagreements they exhibit, will undoubtedly leave the reader with his or her own set of questions. What is the role, or the limit, of historical considerations in dealing with the issue of demarcation? Has one criterion of demarcation in particular, or one general approach, been shown preferable to its rivals? What is it to confirm a theory, and what types of theory are confirmable? Is demarcation in Popper's sense possible at all?

Our questions started with an examination of astrology in the previous section. In the following sections we will pursue these general questions further by examining other controversial topics in more detail: parapsychology, UFOs, and ancient astronauts.

Winning through Pseudoscience

CLARK GLYMOUR AND DOUGLAS STALKER

Like you, we used to think that enterprise, diligence, and a positive mental attitude would make sure we got ahead, into the big bucks. Like you, perhaps, we struggled along on sixty grand a year, worrying about cash flow, complaining about taxes, never making the big bucks. Then we got wise, and after we got wise—well, we got rich. Now we're going to share our methods because, frankly, we can afford to.

To get to the big bucks, you need a gadget. There are lots of gadgets, but brains and guts won't get you most of them. A rich uncle is a gadget, especially if he dies and leaves it all to you, but you've either got a rich uncle or you haven't, and there's not much you can do about it either way. But there's one gadget that's available to anyone with a high school diploma, anyone who is not afraid to use his noggin. Erich von Däniken, Sydney Omarr, Sybil Leek, Bernard Gittelson, Immanuel Velikovsky, and Werner Erhard are doing fine. They have different gadgets, but they all have the same *kind* of gadget: pseudoscience. It sells. People just love to believe, and our research shows that eighty percent of them will believe things a gorilla wouldn't. They'll cheerfully empty their wallets to anyone with a twinkle on his tongue and a pseudoscience in his pocket. Astrology, biorhythms, ESP, numerology, astral projection, scientology, UFOlogy, pyramid power, psychic surgeons, Atlantis real estate—they're all good business. Even Bigfoot turns a buck.

We woke up to the fact that in America pseudoscience is the gadget to give you a leg up. But, you may say, virtually every imaginable pseudoscience already has its promoter, the market is cornered, there's no way for a new boy to break in. Wrong. You break in with your *own* pseudoscience, and we'll show you how. Just to make it easy, we are going to divulge the very principles behind it all. We'll even build two of these money-making

pseudosciences right here, and show you how well they work. If you can't do better by yourself, we'll sell you a franchise in our very own Peruvian Pick-Up Sticks. Once you have your own pseudoscience, or even a franchise (with your own territory, of course), the possibilities for turning it into dollars are endless. To set you off in the right direction, we'll give you some tips, taken from the masters.

Fundamental Principles of Pseudoscience Construction

Building your very own pseudoscience, and even defending it against skeptics, is very easy if you keep a few simple principles in mind.

Principle 1: A coincidence in the hand is worth two in the bush. Start your pseudoscience from a few odd coincidences, then work backward to a preposterous theory that fits them. Some would-be pseudoscience builders make the serious mistake of thinking they must start with firm, clear assumptions and see if they can then obtain amazing predictions from these assumptions. They seldom succeed. Start with the coincidences you want, it's safer and easier. Coincidences are nearly everywhere. But if you have difficulty locating genuine coincidences that you like, start with any event or state of affairs—for example, the fact that you are wearing green socks today. Now trace back the history of the fact far enough, and—hey! presto!—it becomes enormously improbable, an amazing coincidence, that the thing ever happened. After all, what was the chance, eight million years ago, that the universe would evolve so that you are wearing green socks today? Dinky, right? With this technique, you can take any plain-looking fact and turn it into something really startling.

Principle 2: A purpose to everything and everything to its purpose. By faithfully applying Principle 1, you end up with a number of facts that cry out for an explanation. The easiest way to explain those amazing coincidences you have just manufactured is to say that they are no accident at all. They happened on purpose. Whose purpose, what purpose? Yours, dummy. Those green socks signify great things; they portend and presage, they are part of a plan, a cosmic aim or will, some big intention or resolution. Just remember: things don't just happen, they happen on purpose, and it's always your purpose. Pick one and see, but do your choosing after you've read Principle 3, which concerns making the best possible choice in these matters.

Principle 3: The taller the story, the harder it falls. Let your imagination run free when inventing explanations for those amazing coincidences, and give the cold theory a little personal touch, some poignancy, human or inhuman interest. There are lots of models. Oppressed dwarves driven to living inside a hollow earth, daring space travelers dropping human prototypes to rule over the apes (it helps if your theory contradicts a theory nobody much likes anyway), planets that go bump in the night. You can do better: chickens

didn't exist on earth before the year 2000 B.C. That's because they are Martians in disguise, and they can influence the minds of people who eat egg yolks. See, it's easy.

Principle 4: Even physics isn't all that precise. But, you might say, chickens did exist before 2000 B.C. Well, so what? Don't get bogged down with facts. Nobody knows about things like what lived 4,000 years ago, or the laws of celestial mechanics or the theory of probability, except maybe a few stuffy professors that nobody reads anyway. (In case they do get in print somewhere, see Principle 6.) The plain truth about facts is this: if you need a fact, try a fiction. If you need something to be true, say it is.

Principle 5: Science is numbers and gauges. Or so think the unwashed, which means that your pseudoscience will have better sales potential if it makes use of a mysterious device, or a lot of calculations (but *simple* calculations). You have to watch out with the gadgets or the FDA will be after you, but the calculations are safe as pie. They should have some method to them, but not much. A few additions and subtractions, perhaps, and then one long division. The great models are astrology and biorhythms, and, we must say, our own Fascinating Rhythm, which is described below.

Principle 6: Saying no to nit-pickers. The world, alas, is filled with skeptics and troublemakers, some of whom may complain that your theoretical claims contradict well-known scientific results, or are unscientifically vague, or that you lack evidence for your pseudoscience. None of these individuals understands the free enterprise system. Don't be intimidated: follow our advice, show a little grit and you'll reduce them to quivering ectoplasm. Here are five sure-fire strategies.

(a) Tell them they don't understand (especially if they offer a counterexample). When confronted with counterexamples and cavils, make a distinction, any distinction. If really hard pressed, keep changing your claims (but use the same words as much as possible) while insisting that you are only saying what you meant all along.

(b) If the skeptics complain about a lack of evidence, just remember that, so far as the public is concerned, one explanation is worth a thousand tests. Remind them of all of the amazing coincidences your theory explains. How could that not be evidence?

(c) Quote a biggie at them. Emerson is best, that stuff about a foolish consistency being the hobgoblin of little minds (maybe leave out the "foolish").

(d) Wax philosophical. Talk about alternative conceptual schemes, evolving criteria of evidence, methodological opacity, the jiggle between theory and evidence, the necessity to believe in order to understand, the ever-present incommensurability.

(e) Counterattack! Point out that you and your work are victims of the narrow scientific establishment, just like Galileo and his work. Mention

narrow-minded expertise, missing the forest for the trees, government plots, and the like.

Two Pseudosciences from Scratch

As we promised, we will unveil two money-makers right before your very eyes. First off, we explain Fascinating Rhythm, a true analog to biorhythms. It has some distinct advantages, including confirmation, glitter, and advertising appeal (*and* you can dance to it). With FR-3, short for Fascinating Rhythm, you can now chart the syncopated beat of our lives. It's computed in the very same way as that phony bio stuff. You start with your date of birth, and then chart through to the day you want. Here's how.

There are three important cycles in the life of each and every human being. The Mambo cycle runs its full course in 145 days, half on the upswing, half on the downbeat. The Mambo cycle controls your intellectual life, your powers of thinking and deciding. Next, there is the Samba cycle, and it takes a full course in 93 days. Once again, half on your good side, half on your bad. The Samba cycle governs your physical life, those nerves and muscles, that strength and endurance. Last but not least, we have our personal favorite, the Rhumba. Can you feel the beat? The Rhumba cycle goes a mere 48 days. Half up, half down. The Rhumba, as you might guess, regulates your emotional life; it's the meaning behind your fluctuating moods.

To see how well FR-3 works, and to gain a fuller understanding of its powers, we will chart the rhythms for two important people and important events in their lives. So let's do some real science with Wilt Chamberlain and Janis Joplin. On March 2, 1962 Wilt, you may recall, scored 100 points in one game of basketball. Consider what his FR-3 chart tells us about that day. Wilt

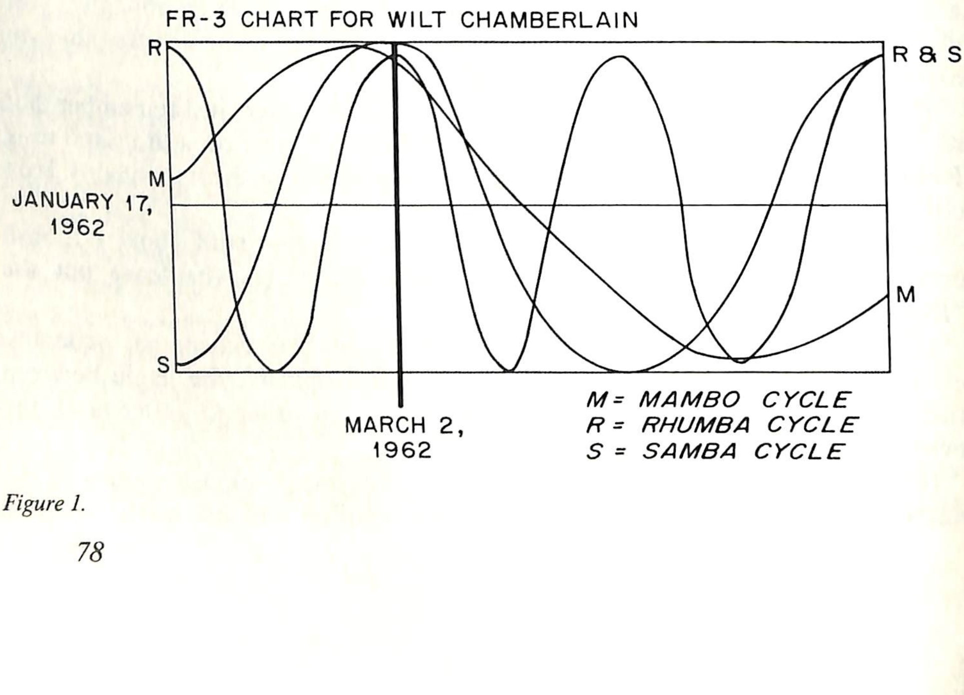


Figure 1.

Chamberlain was born on August 21, 1936. On that fated evening at Madison Square Garden he had been alive 9,324 days. Nine thousand three hundred twenty-four divided by 145 (the number of days in a Mambo cycle) gives 64 full Mambo cycles, with a remainder of 44 days. Nine thousand three hundred twenty-four divided by 93 gives 100 full Samba cycles, with 24 days left over. And divided by 48, 9,324 gives 194 full Rhumba cycles, with 12 days left over. So on March 2, 1962, Wilt the Stilt was 44 days into a Mambo cycle, 24 days into a Samba cycle, and 12 days into a Rhumba cycle. The chart of his rhythms (starting from January 17, 1962) appears in Figure 1. Even a child can see that FR-3 shows *why* Wilt had a great day. All three curves peak, peak, peak. But we know that you want more proof, and so we will draw up a chart for Janis Joplin, too. Recall that Janis was born on January 19, 1943. She died on October 4, 1970. It's easy enough to see what her chart looks like for her last day. She had then been alive 10,120 days. Dividing by the Mambo cycle (145 days), we find that she was 115 days into a new Mambo cycle. The Samba cycle is just as easy to calculate, and for Janis Joplin we find that she had gone through 108 full Samba cycles, which means that she was 76 days into a new Samba cycle. And, last but not least, Joplin had danced 210 full Rhumbas on that fatal day; she was just 40 days into her new Rhumba cycle. This is all plain when you glance at her chart in Figure 2. Once again, FR-3 explains the events. On October 4, 1970, all three of her cycles were close to their lowest, and most dangerous points. If you like, run a biorhythm for these same people and these same events. You will find what we found: FR-3 produces the best chart; it explains events that biorhythms can't. The only rhythms for you, it should be obvious, are the Fascinating ones. We wouldn't tap our toe to any other beat.

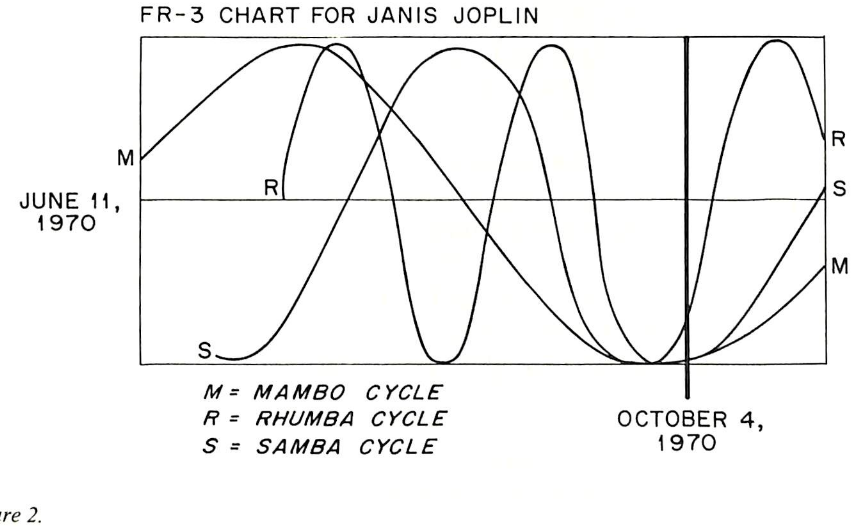


Figure 2.

Once is not enough. And so we describe for you a second, new and profound pseudoscience. It concerns the inner self. Since the beginning of philosophical reflection, the most challenging task has always been to know oneself, and nowadays people want to know themselves before they know you. We have discovered the one sure path to the true person, the way to locate the real them: Alphabetology. It's better than astrology and numerology put together, and easier too.

For centuries, men have noticed the metaphysical and theosophical importance of naming. After all, just look at the mystical and undeniable importance western religions place on baptizing, which concerns only the first name. The more important part of naming—the last name—has been overlooked. By carefully noting three things about your last name, you'll be ready to do a reading that reveals all about you, or anyone else for that matter. So write down your last name—print if you can, that's best for the process. Now you need to find three things in your name: the Vowel Signs, the Ascending Consonant, and the Syllable House. To find your Vowel Signs, locate the vowels in your name, then consult our handy chart:

A	sincere, strong, patient
E	lucky, fluid, engaging
I	proud, forceful, possible born leader
O	aggressive, impatient, fighting instinct
U	gloomy, scholarly, punctual
Y	unpredictable, skillful, natural executive

To find the Ascending Consonant, simply find the first consonant in your last name, and then consult the chart below:

Group 1:	b, c, d, f, g	benefits, advantages
Group 2:	h, j, k, l, m	conflicts, strife
Group 3:	n, p, q, r, s	difficulties, deviousness
Group 4:	t, v, w, x, z	resolutions, conclusions

Don't draw any sweeping conclusions yet. So far, we have only sketched the true person, chosen the paints for the portrait, as it were. A person is not just a collection of traits and happenings. No sir, these items go together, they combine, they dominate or recede. You know that a painting of a person is not just a list of colors. They must go together, and so must the Vowel Signs and Ascendant. How? You find out by finding your Syllable House. To do that, count up the number of syllables in your last name, and then consult our third chart:

1 syllable	commutative (combining independent of order)
2 syllables	distributive (diffusing more or less evenly or unevenly)
3 syllables	associative (relating by groups)

4 syllables	symmetric (having corresponding yet connecting points)
5 syllables	contrapositive (permutating major parts, probably not positive)

These houses, as you no doubt have noted, find their expression in principles that are somewhat like mathematical principles, but not just like them. This difference is important when it comes to dealing with real flesh-and-blood people. It means that our principles cannot be reduced to mechanical operations, principles whose application involves no human judgment. The human factor is ever present and irreducible here, as it is throughout our real lives and activities, in contrast to the fictional world scientists describe for us.

We put these principles into action by making up readings. We have two sample readings, each based on a fully drawn analysis of the names in question, the respective Signs, Houses, and Ascendants, as well as the skill of one of our most practiced readers.

Official Reading for:	Nixon	
Vowel Signs:	i	proud, forceful, born leader
	o	aggressive, impatient, fighting instinct
Ascendant:	n	difficulties, deviousness
House:		distributive

Reading: Nixon is certainly a proud person, as his public appearances and writings clearly indicate. He is also forceful, aggressive, and impatient. These traits were shown time and time again in national and international politics. Nixon, more than most world figures, has a fighting instinct. After all, isn't he still fighting? His Ascending Consonant points to the Watergate affair, among many lesser problems. His Signs and Ascendant seem to distribute evenly, as can be seen even by the untutored eye. Nixon, devious? The reading tells all. So be it!

This reading seems so accurate it is hard to believe, but this is the kind of result you get from Alphabetology, a clearly superior brand of pseudoscience. Quick, accurate readings are its hallmark. For example, just glance at our second sample reading and see how accurate it is.

Official Reading for:	Hitler	
Vowel Signs:	i	proud, forceful, born leader
	e	lucky, fluid, engaging
Ascendant:	h	conflicts, strife
House:		distributive

Reading: Although infamous, Hitler was admittedly proud, forceful, and engaging—a born leader with more than his share of luck to be at the right time to take over a mighty nation. Indeed, an amazingly lucky chain of events that brought Hitler to power, one only a very lucky man could chance on—from housepainter to Fürher; think of it! His changing views and whims and fancies showed him to be fluid in his preferences, most often to his discredit and the dismay of others. One week you could be his friend, the next his enemy.

> Certainly his life was marked with conflicts, so much so that his traits distributed in the direction of definite evil. An uneven total, regrettably. One can only wonder what he might have been if they had coalesced differently.

We would go on to show you more official readings, but this book is pretty high-class, and so the editor wants to save room for some more penetrating articles. Suffice it to say, try out a name, get some practice in before you don your gold chain and go to the bar tonight.

The Importance of Principle 6

The foregoing, we trust, illustrates for you most of the principles of pseudoscience construction. Naturally you will want to apply them in your own original way. Principle 6 is the great thing, however, and to give you a taste of how to use it we offer the following snippet of dialogue, taped while one of us was interviewed by a hostile reporter.

> *Reporter:* Dr. Stalker, there are a lot of well-known scientists who say your Alphabetology is nothing but a sham. There's even a petition going round to try to prevent you from teaching it.
> *Stalker:* What do you expect from people taught by rote? Orthodox scientists are totalitarians at heart, they can't stand nonconformity; as for my colleagues, they know that in the free market of ideas Alphabetology will survive because it is the fittest. That, of course, is why they will stop at nothing to prevent students from voting with their butts . . .
> *Reporter:* Wouldn't Alphabetology forecast the same reading for both Jimmy Carter and Billy Carter? But they're not the same!
> *Stalker:* What makes you think that Jimmy and Billy say "Carter" *exactly* the same way? No chance. You fail to understand the full ramifications of Alphabetology. You've failed to distinguish between long and short vowels, for one thing. A Group 1 Ascending Consonant can, when the Syllable House is distributive, go evenly or unevenly, especially when the Ascending Consonant precedes a short vowel. This can cause a polarity. For example, an A sign might shift to an O sign, or it might remain the same. And, you must remember, we are not omniscient. What mortal or mortal's theory is?
> *Reporter:* What do you say to the criticism that none of your work on Alphabetology contains any properly controlled experiments?
> *Stalker*: Well, what's likelier, that Alphabetology is true or that *just by chance* its principles correctly describe the character of such different people as Nixon, Ghandi, and Hitler? There are hundreds of people whose characters have been correctly deciphered on alphabetological principles: Julius La Rosa, Pablo Casals, T. G. McGonnigle, Irving Thalberg, Mary Ann Mobley, Randolph Scott, Jay Silverheels, Barney Newman, and Carl Andre, just to name a few. What do you think–that all of these analyses hold just by accident? Piffle.

We hope this excerpt helps you put Principle 6 into action. With the world filled with so many depressed and destructive types, you need to be adept with Principle 6. If you still feel uncertain about how to use the principle, we recommend a careful study of the *Playboy* interview with Erich von Däniken.

Cashing In

Once you have your pseudoscience, how is it to be turned into hard dollars? Write a book, and entitle it something like *My Struggles to Reveal the Scientific Basis of . . .*, or *Winning Through . . .* If you can't write a book, make a dingy device to go with your pseudoscience and market it. For example, for only $49.95 our own Peruvian Pick-Up Sticks comes with genuine Peruvian pick-up sticks and board, and an authentic Peruvian Protractor. As for outlets, first try mail-order magazines like *Argosy, Fate,* and *Popular Mechanics*. And, don't forget, your local head shop will be glad to take a few on consignment. If you're full of energy, start a weekend workshop on your pseudoscience. Advertise a little and the post-teenyboppers will flock around you, just as though you were Travis McGee. Avoid sex at first—remember, this is science—but give just the hint of Future Sex. That'll bring 'em back for the next workshop. Before long, you'll be on the radio talk shows, and even daytime TV, along with the accordion players and Tupperware saleswomen.

If you give it an honest try and you fail, either because of difficulties with your pseudoscience, or because of marketing difficulties, then do the simple thing: take out a franchise with us. Our Peruvian Pick-up Sticks has been market tested in Portland, Oregon with astounding success. Right now, we're selling franchises faster than we can draw up the proper papers. But, frankly, we want even more money than we have now, and that means you have a chance to buy your very own franchise. It comes with a pseudoscience starter kit that enables you, in one short weekend of occasional practice, to turn a dollar on Monday. On Tuesday, the word will be out, your flag will be flying, and you may be on your way to untold millions.

Your starter kit comes complete with a magnificent tall tale about the cosmic significance of Peru and its sticks. To quote only a small portion of the spiel (written by no less an expert than Spiro Agnew's nephew):

> In deepest, darkest Peru, the naïve natives find themselves puzzled, even placed in a pickle, by certain massive indentations in the earth. Unsound scholars have speculated that these superficial scrawls were made to guide aliens to safe berth in any storm. The truth is now revealed. These markings, these scrawls, are but reflections of the king's castings in the game of fate, in the game of life, in the game of death known to us of late as Peruvian Pick-up Sticks.

Pretty good, huh? It works a lot like mood music by Montavani, sets the atmosphere, gets the good vibes going and the thinking to a minimum.

Your starter kit also comes with a genuine Peruvian Pick-up Sticks board, which is made of laminated Beau d'Arc wood. The board measures a full 12″ by 12″. In each corner of the board is a symbol signifying the essence and concrescence of human aspiration and hominoid degradation. Up in the left corner, there is a bird, signifying power, freedom, and imagination. Over in the upper right corner, you will find a squirrel, which signifies timorousness,

PERUVIAN PICK-UP STICKS BOARD

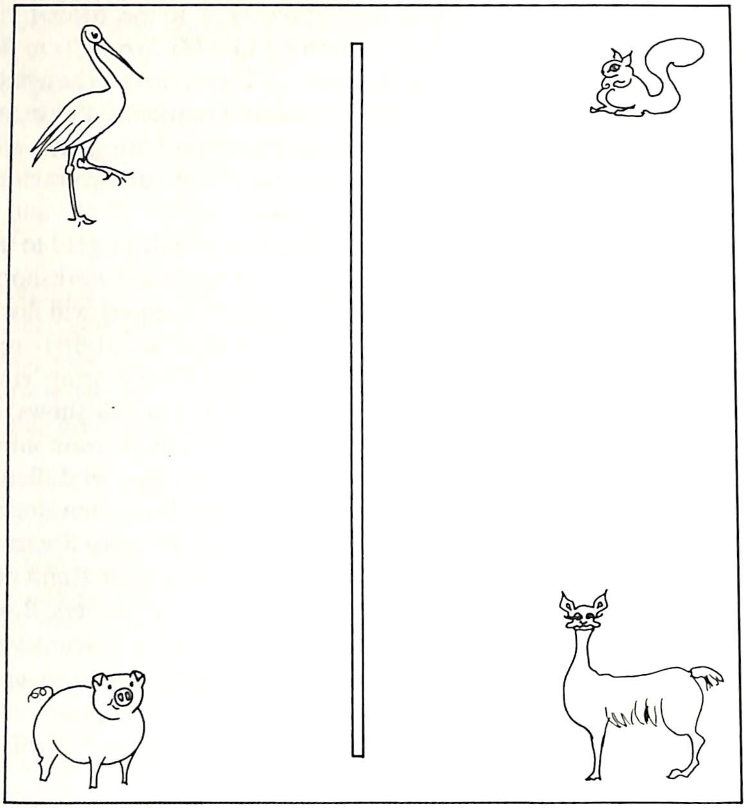


Figure 3.

deep secrets, and an interest in nuts. Down in the lower right corner is a llama, signifying tenacity, patience, duration, and valuable hair. And last, over in the lower left corner, you can see a pig, which signifies solidity, firmness, character, and a deep attachment to the present. In addition to these figures (drawn in real enamel), the board contains a groove, indeed the Great Groove, which is a direct representation of the actual huge and great groove that scars the surface of Peru. The board is illustrated in Figure 3. In real life, and for longer wear, it is covered with a tough vinyl coating. We'll even throw in a nice-looking Naughahide carrying case, and put your initials on the flap.

Besides the board, Peruvian Pick-up Sticks requires three sticks, preferably made of thorns from the Beau d'Arc tree (although other materials may, in a pinch, be substituted). Frankly, we're in a pinch, what with these franchises selling like hotcakes. So your three sticks are made of solid pine, a good wood in its own right. Two of these sticks are called nebishes and may be of any color, though puce is our standard. The third is thicker than the other two,

and it *must*—for obvious reasons—be black. Called "the Wong," it is the stick of premier importance. The three sticks are cast on the board in the manner of the children's game, pick-up sticks. The cast must be made by the person whose fate is to be read, but the reader of the cast may be the caster or some other person of suitable skill. In any case, an accurate reading requires full and, in these times of inflation, somewhat expensive training. To be sure, we have workshops all over the country, especially in California, at which we offer proper training all in one short weekend. Besides, we usually hold them at the Holiday Inn closest to the local airport, so you can get in some dandy socializing along the way.

So, you're no doubt asking, how do you play this game of fate, this game of life, this game of death known to us as Peruvian Pick-up Sticks? It's easy. A cast is made, and then you take out your genuine Peruvian Protractor, which also comes in your starter kit. Just as life is a bunch of angles, so is this game. You find three angles: the angles between each of the nebish sticks and the Great Groove, and the angle that indicates the lie of the Wong (a misnomer

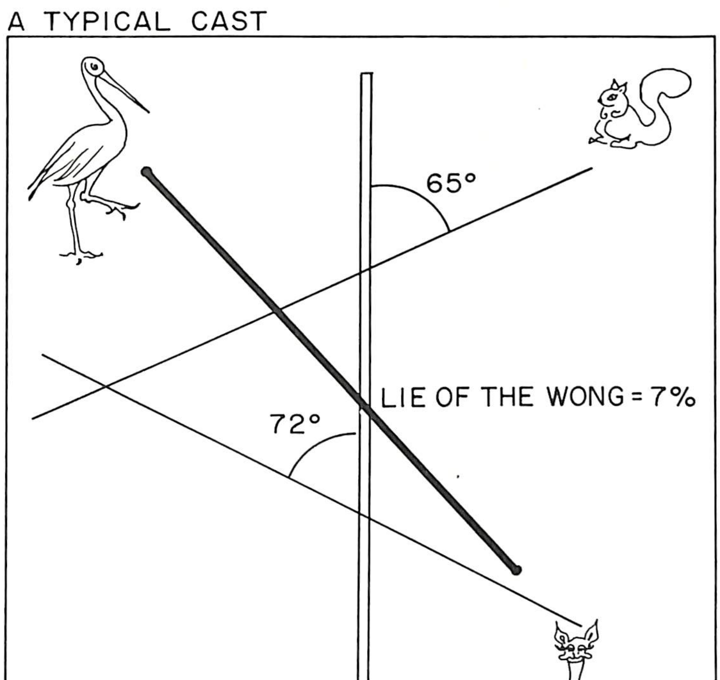


Figure 4.

to be sure, for the Wong does not lie). A typical cast is shown in Figure 4 complete with the angles indicated. In this case, the bird–llama line, which lies closest to the lie of the Wong, is said to *dominate* the cast; the domination is ameliorated, of course, by how far the Wong lies from the dominant line. Having ascertained these angles and the domination, the reader notes (1) the smallest nebish angle; (2) the largest nebish angle; (3) the sum of the nebish angles, and (4) the difference of the nebish angles. The first—the smallest nebish angle—concerns weakness and decay, the uncertainty of the future; the second concerns wisdom, power, and love; the third concerns material success and creativity; and the fourth is a measure of erotic magnetism, spirituality, and tragic prospects. Of course these indicators, these angles, cannot be interpreted mechanically; a skilled reader must have a thorough mastery of the ancient Peruvian principles, and of the many models left to us by the vanished master. These principles and models have been collected by us in a volume forthcoming from a distinguished university press and titled, in faithful translation from the Mayan, *My Thing*. Until the big-press edition hits the stands, we are including our vanity edition in your starter kit. Although not as handsome as the forthcoming edition, it will get you going. After all, why wait, the money is out there, so shouldn't you be?

It would be unprofessional of us to reveal the true reading of the cast we have illustrated. Some of our clients are international celebrities of one sort or another, and they have a right to their privacy, too. We can, however, tell you, merely as a hint, that the cast illustrated was thrown by a distinguished former monarch, that it is dominated by power and imagination, developed patiently and by an extreme tenacity of character (the Wong lying nearly on the llama); that, although the Wong may indicate the fixed character of the caster, the lay of the nebish sticks determines how this character will meet with circumstance. In the present case, the lay of the nebish sticks suggests, most strongly, weakness and decay, not to mention uncertainty. It should be noted that only a month after making the cast we have illustrated the unfortunate monarch was required by his countrymen to take an extended vacation in Morocco.

There you have it. And you can really have it, a full starter kit for only $49.95. Think of it: your own business at last, being your own boss; you'll be celebrated, and toasted, but most important, you'll be getting your piece of the big cake. By invention or by franchise, pseudoscience is your way to riches. We can think of no better envoi than the one L. Ron Hubbard (one of the great pseudoscientists of our time) used to end his classic best seller, *Dianetics: The Modern Science of Mental Health*. After 500 pages on topics from prenatal experiences to using your engrams, Ron put it all in a nutshell when he wrote: "Good hunting!"

Science: Conjectures and Refutations

Karl R. Popper

Mr. Turnbull had predicted evil consequences, . . . and was now doing the best in his power to bring about the verification of his own prophecies.
—Anthony Trollope

I

When I received the list of participants in this course and realized that I had been asked to speak to philosophical colleagues I thought, after some hesitation and consultation, that you would probably prefer me to speak about those problems which interest me most, and about those developments with which I am most intimately acquainted. I therefore decided to do what I have never done before: to give you a report on my own work in the philosophy of science, since the Autumn of 1919 when I first began to grapple with the problem, "*When should a theory be ranked as scientific?*" or "*Is there a criterion for the scientific character or status of a theory?*"

The problem which troubled me at the time was neither, "When is a theory true?" nor, "When is a theory acceptable?" My problem was different. *I wished to distinguish between science and pseudo-science*; knowing very well that science often errs, and that pseudo-science may happen to stumble on the truth.

I knew, of course, the most widely accepted answer to my problem: that science is distinguished from pseudo-science—or from "metaphysics"—by its *empirical method*, which is essentially *inductive*, proceeding from observation or experiment. But this did not satisfy me. On the contrary, I often formu-

A lecture given at Peterhouse, Cambridge, in summer 1953, as part of a course on developments and trends in contemporary British philosophy, organized by the British Council; originally published under the title "Philosophy of Science: a Personal Report" in *British Philosophy in Mid-Century*, ed. C. A. Mace, 1957.

lated my problem as one of distinguishing between a genuinely empirical method and a non-empirical or even a pseudo-empirical method—that is to say, a method which, although it appeals to observation and experiement, nevertheless does not come up to scientific standards. The latter method may be exemplified by astrology, with its stupendous mass of empirical evidence based on observation—on horoscopes and on biographies.

But as it was not the example of astrology which led me to my problem I should perhaps briefly describe the atmosphere in which my problem arose and the examples by which it was stimulated. After the collapse of the Austrian Empire there had been a revolution in Austria: the air was full of revolutionary slogans and ideas, and new and often wild theories. Among the theories which interested me Einstein's theory of relativity was no doubt by far the most important. Three others were Marx's theory of history, Freud's psycho-analysis, and Alfred Adler's so-called "individual psychology."

There was a lot of popular nonsense talked about these theories, and especially about relativity (as still happens even today), but I was fortunate in those who introduced me to the study of this theory. We all—the small circle of students to which I belonged—were thrilled with the result of Eddington's eclipse observations which in 1919 brought the first important confirmation of Einstein's theory of gravitation. It was a great experience for us, and one which had a lasting influence on my intellectual development.

The three other theories I have mentioned were also widely discussed among students at that time. I myself happened to come into personal contact with Alfred Adler, and even to co-operate with him in his social work among the children and young people in the working-class districts of Vienna where he had established social guidance clinics.

It was during the summer of 1919 that I began to feel more and more dissatisfied with these three theories—the Marxist theory of history, psycho-analysis, and individual psychology; and I began to feel dubious about their claims to scientific status. My problem perhaps first took the simple form, "What is wrong with Marxism, psycho-analysis, and individual psychology? Why are they so different from physical theories, from Newton's theory, and especially from the theory of relativity?"

To make this contrast clear I should explain that few of us at the time would have said that we believed in the *truth* of Einstein's theory of gravitation. This shows that it was not my doubting the *truth* of those other three theories which bothered me, but something else. Yet neither was it that I merely felt mathematical physics to be more *exact* than the sociological or psychological type of theory. Thus what worried me was neither the problem of truth, at that stage at least, nor the problem of exactness or measurability. It was rather that I felt that these other three theories, though posing as science, had in fact more in common with primitive myths than with science; that they resembled astrology rather than astronomy.

I found that those of my friends who were admirers of Marx, Freud, and Adler, were impressed by a number of points common to these theories, and especially by their apparent *explanatory power*. These theories appeared to be able to explain practically everything that happened within the fields to which they referred. The study of any of them seemed to have the effect of an intellectual conversion or revelation, opening your eyes to a new truth hidden from those not yet initiated. Once your eyes were thus opened you saw confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it. Thus its truth appeared manifest; and unbelievers were clearly people who did not want to see the manifest truth; who refused to see it, either because it was against their class interest, or because of their repressions which were still "un-analysed" and crying aloud for treatment.

The most characteristic element in this situation seemed to me the incessant stream of confirmations, of observations which "verified" the theories in question; and this point was constantly emphasized by their adherents. A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history; not only in the news, but also in its presentation—which revealed the class bias of the paper—and especially of course in what the paper did *not* say. The Freudian analysts emphasized that their theories were constantly verified by their "clinical observations." As for Adler, I was much impressed by a personal experience. Once, in 1919, I reported to him a case which to me did not seem particularly Adlerian, but which he found no difficulty in analysing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how he could be so sure. "Because of my thousandfold experience," he replied; whereupon I could not help saying: "And with this new case, I suppose, your experience has become thousand-and-one-fold."

What I had in mind was that his previous observations may not have been much sounder than this new one; that each in its turn had been interpreted in the light of "previous experience," and at the same time counted as additional confirmation. What, I asked myself, did it confirm? No more than that a case could be interpreted in the light of the theory. But this meant very little, I reflected, since every conceivable case could be interpreted in the light of Adler's theory, or equally of Freud's. I may illustrate this by two very different examples of human behaviour: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child. Each of these two cases can be explained with equal ease in Freudian and in Adlerian terms. According to Freud the first man suffered from repression (say, of some component of his Oedipus complex), while the second man had achieved sublimation. According to Adler the first man suffered from feelings of inferiority (producing perhaps the need to prove to himself that he dared to commit

some crime), and so did the second man (whose need was to prove to himself that he dared to rescue the child). I could not think of any human behaviour which could not be interpreted in terms of either theory. It was precisely this fact—that they always fitted, that they were always confirmed—which in the eyes of their admirers constituted the strongest argument in favour of these theories. It began to dawn on me that this apparent strength was in fact their weakness.

With Einstein's theory the situation was strikingly different. Take one typical instance—Einstein's prediction, just then confirmed by the findings of Eddington's expedition. Einstein's gravitational theory had led to the result that light must be attracted by heavy bodies (such as the sun), precisely as material bodies were attracted. As a consequence it could be calculated that light from a distant fixed star whose apparent position was close to the sun would reach the earth from such a direction that the star would seem to be slightly shifted away from the sun; or, in other words, that stars close to the sun would look as if they had moved a little away from the sun, and from one another. This is a thing which cannot normally be observed since such stars are rendered invisible in daytime by the sun's overwhelming brightness; but during an eclipse it is possible to take photographs of them. If the same constellation is photographed at night one can measure the distances on the two photographs, and check the predicted effect.

Now the impressive thing about this case is the *risk* involved in a prediction of this kind. If observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is *incompatible with certain possible results of observation*—in fact with results which everybody before Einstein would have expected.[1] This is quite different from the situation I have previously described, when it turned out that the theories in question were compatible with the most divergent human behaviour, so that it was practically impossible to describe any human behaviour that might not be claimed to be a verification of these theories.

These considerations led me in the winter of 1919–20 to conclusions which I may now reformulate as follows.

(1) It is easy to obtain confirmations, or verifications, for nearly every theory—if we look for confirmations.

(2) Confirmations should count only if they are the result of *risky predictions*; that is to say, if, unenlightened by the theory in question, we should have expected an event which was incompatible with the theory—an event which would have refuted the theory.

(3) Every "good" scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is.

(4) A theory which is not refutable by any conceivable event is non-scientific. Irrefutability is not a virtue of a theory (as people often think) but a vice.

(5) Every genuine *test* of a theory is an attempt to falsify it, or to refute it. Testability is falsifiability; but there are degrees of testability: some theories are more testable, more exposed to refutation, than others; they take, as it were, greater risks.

(6) Confirming evidence should not count *except when it is the result of a genuine test of the theory*; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory. (I now speak in such cases of "corroborating evidence.")

(7) Some genuinely testable theories, when found to be false, are still upheld by their admirers—for example by introducing *ad hoc* some auxiliary assumption, or by re-interpreting the theory *ad hoc* in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering, its scientific status. (I later described such a rescuing operation as a "*conventionalist twist*" or a "*conventionalist stratagem.*")

One can sum up all this by saying that *the criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.*

II

I may perhaps exemplify this with the help of the various theories so far mentioned. Einstein's theory of gravitation clearly satisfied the criterion of falsifiability. Even if our measuring instruments at the time did not allow us to pronounce on the results of the tests with complete assurance, there was clearly a possibility of refuting the theory.

Astrology did not pass the test. Astrologers were greatly impressed, and misled, by what they believed to be confirming evidence—so much so that they were quite unimpressed by any unfavourable evidence. Moreover, by making their interpretations and prophecies sufficiently vague they were able to explain away anything that might have been a refutation of the theory had the theory and the prophecies been more precise. In order to escape falsification they destroyed the testability of their theory. It is a typical soothsayer's trick to predict things so vaguely that the predictions can hardly fail: that they become irrefutable.

The Marxist theory of history, in spite of the serious efforts of some of its founders and followers, ultimately adopted this soothsaying practice. In some of its earlier formulations (for example in Marx's analysis of the character of the "coming social revolution") their predictions were testable, and in fact falsified.[2] Yet instead of accepting the refutations the followers of Marx re-interpreted both the theory and the evidence in order to make them agree. In this way they rescued the theory from refutation; but they did so at the price of adopting a device which made it irrefutable. They thus gave a "conventionalist twist" to the theory; and by this stratagem they destroyed its much advertised claim to scientific status.

The two psycho-analytic theories were in a different class. They were simply non-testable, irrefutable. There was no conceivable human behaviour which could contradict them. This does not mean that Freud and Adler were not seeing certain things correctly: I personally do not doubt that much of what they say is of considerable importance, and may well play its part one day in a psychological science which is testable. But it does mean that those "clinical observations" which analysts naively believe confirm their theory cannot do this any more than the daily confirmations which astrologers find in their practice.[3] And as for Freud's epic of the Ego, the Super-ego, and the Id, no substantially stronger claim to scientific status can be made for it than for Homer's collected stories from Olympus. These theories describe some facts, but in the manner of myths. They contain most interesting psychological suggestions, but not in a testable form.

At the same time I realized that such myths may be developed, and become testable; that historically speaking all—or very nearly all—scientific theories originate from myths, and that a myth may contain important anticipations of scientific theories. Examples are Empedocles' theory of evolution by trial and error, or Parmenides' myth of the unchanging block universe in which nothing ever happens and which, if we add another dimension, becomes Einstein's block universe (in which, too, nothing ever happens, since everything is, four-dimensionally speaking, determined and laid down from the beginning). I thus felt that if a theory is found to be non-scientific, or "metaphysical" (as we might say), it is not thereby found to be unimportant, or insignificant, or "meaningless," or "nonsensical."[4] But it cannot claim to be backed by empirical evidence in the scientific sense—although it may easily be, in some genetic sense, the "result of observation."

(There were a great many other theories of this pre-scientific or pseudo-scientific character, some of them, unfortunately, as influential as the Marxist interpretation of history; for example, the racialist interpretation of history—another of those impressive and all-explanatory theories which act upon weak minds like revelations.)

Thus the problem which I tried to solve by proposing the criterion of falsifiability was neither a problem of meaningfulness or significance, nor a problem of truth or acceptability. It was the problem of drawing a line (as well as this can be done) between the statements, or systems of statements, of the empirical sciences, and all other statements—whether they are of a religious or of a metaphysical character, or simply pseudo-scientific. Years later—it must have been in 1928 or 1929—I called this first problem of mine the "*problem of demarcation*." The criterion of falsifiability is a solution to this problem of demarcation, for it says that statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations.

Notes

1. This is a slight oversimplification, for about half of the Einstein effect may be derived from the classical theory, provided we assume a ballistic theory of light.

2. See, for example, my *Open Society and Its Enemies*, ch. 15, section iii, and notes 13–14.

3. "Clinical observations," like all other observations, are *interpretations in the light of theories*; and for this reason alone they are apt to seem to support those theories in the light of which they were interpreted. But real support can be obtained only from observations undertaken as tests (by "attempted refutations"); and for this purpose *criteria of refutation* have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst not merely a particular analytic diagnosis but psycho-analysis itself? And have such criteria ever been discussed or agreed upon by analysts? Is there not, on the contrary, a whole family of analytic concepts, such as "ambivalence" (I do not suggest that there is no such thing as ambivalence), which would make it difficult, if not impossible, to agree upon such criteria? Moreover, how much headway has been made in investigating the question of the extent to which the (conscious or unconscious) expectations and theories held by the analyst influence the "clinical responses" of the patient? (To say nothing about the conscious attempts to infuence the patient by proposing interpretations to him, etc.) Years ago I introduced the term "*Oedipus effect*" to describe the influence of a theory or expectation or prediction *upon the event which it predicts* or describes: it will be remembered that the causal chain leading to Oedipus' parricide was started by the oracle's prediction of this event. This is a characteristic and recurrent theme of such myths, but one which seems to have failed to attract the interest of the analysts, perhaps not accidentally. (The problem of confirmatory dreams suggested by the analyst is discussed by Freud, for example in *Gesammelte Schriften*, III, 1925, where he says on p. 314: "If anybody asserts that most of the dreams which can be utilized in an analysis . . . owe their origin to the analyst's suggestion, then no objection can be made from the point of view of analytic theory. Yet there is nothing in this fact," he surprisingly adds, "which would detract from the reliability of our results.")

4. The case of astrology, nowadays a typical pseudo-science, may illustrate this point. It was attacked, by Aristotelians and other rationalists, down to Newton's day, for the wrong reason—for its now accepted assertion that the planets had an "influence" upon terrestrial ("sublunar") events. In fact Newton's theory of gravity, and especially the lunar theory of the tides, was historically speaking an offspring of astrological lore. Newton, it seems, was most reluctant to adopt a theory which came from the same stable as for example the theory that "influenza" epidemics are due to an astral "influence." And Galileo, no doubt for the same reason, actually rejected the lunar theory of the tides; and his misgivings about Kepler may easily be explained by his misgivings about astrology.

Demarcating Genuine Science from Pseudoscience

DANIEL ROTHBART

Most of us believe that a scientific theory differs fundamentally from a pseudoscientific theory. Darwinism and the divine theory of creation differ not simply in their respective content but in the way they each explain the phenomena. Astronomy and astrology clash in a way that prompts us to classify the former as genuine science and the latter as pseudoscience. The abundance of paradigm cases of science and pseudoscience may suggest that the defining characteristics of genuine science are easily within our grasp, and that the dividing line between science and non-science is so obvious as to be unworthy of serious philosophical effort.

But in the opinion of many philosophers, no proposal to date satisfactorily demarcates genuine science from pseudoscience, for none successfully captures the common strands underlying the paradigm "scientific" theories. Each proposal seems to clash repeatedly with clear cases of either science or pseudoscience. To aggravate the problem, many theories such as Freudian psychoanalysis and Marxism are not uncontroversially classifiable as either science or pseudoscience, prompting heated debates between advocates and critics of the particular theory. In order to be successful, a demarcation criterion must enable us to distinguish the paradigm cases of genuine science from those of pseudoscience, as well as clearly to settle the classification of the many doubtful cases. [1]

The purpose of this paper is to propose and defend a demarcation criterion. After first showing why Karl Popper's famous demarcation criterion fails, I offer an alternative criterion that is far more historically oriented than Popper's. According to the alternative criterion, a theory's classification as science or non-science rests significantly on rival theories under consideration at a particular time.

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Popper on Demarcation

In his well-known attempt to solve the demarcation problem, Popper contends that the defining feature of science is its falsifiability, that is, its vulnerability to refutation by at least one possible event. A theory is scientific on this view if there exists some *possible* state of affairs whose actual occurrence would refute the theory. According to Popper, this potential for refutation reveals that the theory clashes with experience and so is an instance of empirical science. Newtonian mechanics is scientific since the occurrence of many possible events would falsify the theory, such as the failure of Halley's comet to return to earth around the calculated time of December 1758. Conversely, any theory immune to falsification by some possible event is by definition pseudoscientific on this view, since these theories fail to clash with experience, as illustrated by tautologous theories that are empirically empty (Popper, 1959, Chs. 1, 4).

But clearly, this criterion yields an impoverished view of science, and is far too weak to function as a defining standard. The criterion systematically classifies a wide range of pseudoscientific theories as instances of science (Agassi, 1964, p. 198). To satisfy Popper's falsifiability criterion, a theory need simply describe any one specific state of affairs by implying just one observation statement, or basic statement.[2] The basic statement need not actually be false, but were it false then the theory *would* be refuted. So on this view any theory is scientific if it yields *any observation statement* whatsoever, so long as the theory would be refuted if that statement happened to be false. It does not matter whether the statement is true or false, or whether the statement describes a past event (as in explanations) or a future event (as in predictions). But these conditions for science are far too weak, for they open the gates to a seemingly endless series of pseudoscientific theories that are falsifiable. Clearly, very many pseudoscientific theories have at least *some* contact with the world by describing at least *one* possible event. Velikovsky's theories make explicit reference to planetary orbits and specific terrestrial events; the theory describing dowsing rods to find underground water yields empirical predictions; the theory of divine creation is not only falsifiable by many possible events but has in fact been repeatedly falsified, as have witchcraft and voodoo (Gardner, 1952). Astrology as well has been empirically falsified, which reveals its falsifiable character (Jerome, 1975).[3] We can easily conjure up bizarre or ridiculous theories that satisfy the falsifiability standard by describing *some* possible event. A full account of an *Alice in Wonderland* universe purportedly describes certain specific events and therefore is falsifiable. So it appears that a successful demarcation criterion must provide for more restrictive standards for science than Popper recommends.

Criteria for a Demarcation Criterion

How might we construct a more plausible criterion of demarcation? Although Popper's criterion fails, he suggests a rather promising approach to solving the demarcation problem, by arguing that the demarcation between science and pseudoscience is to be drawn in terms of methodological characteristics of science, or on the basis of our theory of scientific knowledge (Musgrave, 1968, pp. 80–82). This much I will borrow from Popper since my theory, like his, will rely on a sketch of proper scientific methodology. By drawing the demarcation line in terms of certain methodological rules, I am assuming that genuine science by definition satisfies these methodological standards whereas pseudoscience by definition does not, that Ptolemaic astronomy, for example, meets certain requirements for rational science that superstition lacks. But exactly what type of methodological requirements are relevant in settling the demarcation issue, and what type of requirements can be dismissed as irrelevant?

At this point it may be helpful to distinguish between two types of methodological standards: (1) those testing standards used during and after the experimental test, such as repeatability of experimental results, versus (2) those testing standards used before any testing to determine which theory should be selected for testing. The latter pre-testing standards are indispensable, since scientists are, in principle, barraged by an infinite number of possible theories, which must be ordered by weighing strengths and weaknesses. In selecting a hypothesis for testing, scientists try to maximize certain methodological virtues, which usually suggest future success, and minimize methodological vices, which indicate possible failings. A rational procedure is needed for this a priori selection, that is, the selection that logically precedes testing (Hanson, 1972, Ch. 4).

Within the category of pre-testing requirements, a further distinction can be made between two types of pre-testing standards. The first type includes only those standards that *must* be fulfilled in order for a hypothesis to be rationally selected for testing. These can be called necessary requirements for a priori selection, as illustrated by the condition that, trivially, each hypothesis must produce test statements purportedly describing some future event. Any hypothesis that fails to yield such statements is immune to experimentation and clearly must not—and in a sense cannot—be selected for testing. Let us use the term "eligibility requirements" for all necessary pre-testing requirements, since they indicate whether a hypothesis is an eligible candidate for a priori selection, or whether it is viable for testing by displaying enough promise to be seriously considered for testing.

The second group exhausts all other pre-testing requirements, and consists of all pre-testing standards that, however important, are not strictly necessary for a priori selection for testing. For example, some philosophers maintain

that one standard for a priori selection is the theory's concordance with the accepted background theories at a particular time. But this condition does not seem to be necessary for testing since other virtues of a theory may, according to these philosophers, counterbalance a lack of concordance with background knowledge (Schaffner, 1970, pp. 320–321). Simplicity as well has been widely heralded as a standard for a theory's selection, but again not a necessary requirement, since other factors, such as explanatory power, may warrant the theory's selection despite a lack of simplicity.

My purpose in distinguishing between necessary pre-testing requirements (eligibility requirements) and non-necessary pre-testing requirements is to suggest that it is the former that serve as the basis for a demarcation criterion. I thus propose that the demarcation between science and non-science is the satifaction of all eligibility requirements (all necessary requirements) from our methodology for "rational science."[4] On this view the defining attribute of genuine science is its candidacy as a viable hypothesis for testing or its plausibility to be selected for experimentation–in a word, its *testworthiness*. Scientific theories must exhibit enough merit and promise to render them serious contenders for a priori selection. Although a scientific theory need not be the best testing candidate, it must be a viable candidate by satisfying all eligibility requirements. Conversely, the defining feature of non-science is its implausibility for testing, that is, its failure to meet at least one of the eligibility requirements. Witchcraft, magic, and voodoo are unscientific because they are *unworthy* of experimentation.[5]

Note that my proposal is not by itself a demarcation criterion, but is instead a requirement or adequacy condition for every plausible demarcation criterion, stipulating that the dividing line between science and non-science is the fulfillment of the eligibility requirements, whatever they happen to be. A demarcation criterion can be explicitly suggested only after we agree on the particular eligibility requirements, a problem that I address below. Thus my proposal might be described as a metacriterion for every demarcation criterion.

One argument in behalf of this metacriterion is that it complies with certain intuitions about what distinguishes genuinely scientific systems. The defining feature of science does not seem to be experimental success, for most clear cases of genuine science have been experimentally falsified. It seems instead to be some set of characteristics that theories share prior to testing that makes them scientific. Other theories, such as magic, seem to be classified as unscientific not because of experimental failure but because of some a priori deficiency. Conforming to these intuitions, the metacriterion outlined above maintains that the defining features of science rest exclusively on certain pre-testing requirements–the eligibility requirements–and that the deficiency of all unscientific systems is their untestworthy character.

Distinguishing Science from Non-Science

As noted above, the metacriterion cannot by itself settle the demarcation question without some specification of particular eligibility requirements. My immediate task is to suggest certain eligibility requirements which, in conjunction with the metacriterion, yield a satisfactory demarcation between genuine science and pseudoscience.

To determine the eligibility requirements, which are the pre-testing conditions every hypothesis must satisfy to be a viable testing candidate, consider what function an experimental test is intended to serve. One function that a test does *not* serve is to verify, in an absolute and final sense, the tested hypothesis. Hume and others have shown the impossibility of absolute verification in science. Another spurious function of a test is absolute falsification, which is the final and conclusive rejection of a hypothesis allegedly on empirical grounds. Pierre Duhem has shown that any hypothesis, however suspect, can be salvaged by "blaming" one of the linking auxiliary hypotheses (1962, Ch. 6). Absolute falsification in science is a myth.[6]

Rather than producing some final decision on a hypothesis, by either absolute verification or absolute falsification, the primary function of a test is to contribute to the preferential ranking for the hypothesis in relation to some rival theory. Testing provides empirical grounds for an epistemic ordering of rival hypotheses. Scientists usually perform tests in a context of a competitive struggle between some new hypothesis and some previously accepted background theory. Tests involving the theories of Darwin, Pasteur, and Mendel illustrate this competition in biology, as do tests from Galileo to Newton, from Faraday to Einstein, and from Rutherford to Heisenberg in physics. Tests can serve to justify the preference of one theory over competitors by revealing the empirical advantages of one theory with respect to the present state of scientific discussion (Popper, 1972, pp. 82–84). As a comparative matter, experimental success contributes favorably to the preferential ranking of the theory in relation to alternatives (Lakatos, 1970; Putnam, 1962). But no theory can be conclusively verified or can achieve a status of final success. Experimental refutation diminishes the theory's standing, but only in relation to rival theories at the time of testing. This is illustrated repeatedly in science by "crucial experiments," which lower the comparative standing of one theory in relation to its rival. But few theories have been falsified in isolation, since falsification usually requires some rival theory to fall back on (Lakatos, 1970, p. 179), and since the decision to abandon a theory usually involves a commitment to develop and examine an adequate alternative (Laudan, 1977, pp. 55–57).

Given this role for testing, we can expect the eligibility requirements for a priori selection to reflect the importance of alternative theories at a particular time. As a first eligibility requirement we might propose that the hypothesis selected for testing must account for all the phenomena that its rival back-

ground theory explains; that the hypothesis must encapsulate its rival's explanatory successes. If we have some theory on hand that solves an empirical problem, then any competing hypothesis must match this success in order to maintain its testworthiness. After Galilean mechanics explained the motion of falling bodies, any viable theory in the same domain had to account for such phenomena. If a hypothesis fails to match the explanatory successes of its rivals, the hypothesis shows no improvement, at least in relation to the known evidence, and thus fails to display enough promise to warrant testing. The goal that one theory should accumulate a rival's empirical successes is widely illustrated in science and defended by many philosophers and scientists (Post, 1971, pp. 228–235).[7]

The notion of explanation assumed here requires the following: (a) that the explanans in conjunction with statements of initial conditions imply the explanandum, (b) that the explanans include a lawlike generalization, and (c) that the explanans have some direct contact with the empirical phenomena.[8] Since our discussion concerns explanans with untested hypotheses, one condition that this notion of explanation does not include is that the explanans must be confirmed, corroborated, or true. Because of this exclusion, the notion of explanation assumed above more accurately defines a potential explanation as opposed to a successful explanation.

By grounding viability for testing in the theory's relationship to rival theories, however, this first eligibility requirement does *not* demand that a hypothesis must explain all known facts at a certain time. Few theories ever explain all the facts at a given time, for many theories fail to explain those events that their rivals could not explain (Feyerabend, 1975, pp. 55–60). For example, although mechanical theories of dynamics in the eighteenth century could not explain electromagnetic phenomena, scientists still tested many of these theories. A theory then may be allowed to fail empirically where its older rival theory failed, but still be viable for testing.

In addition to encapsulating the explanatory success of its rival, a viable theory must satisfy a second eligibility requirement. A viable theory must clash empirically with its rival by yielding test implications that are inconsistent with the rival theory. In Popper's terms, it must be susceptible to a severe test whose test statements clash with the given background knowledge accepted at the time of testing (1959, Ch. 10). This requirement is needed to ensure that a testworthy hypothesis has the potential to surpass empirically the background theories; to succeed where alternative theories fail. If a hypothesis fails to meet this requirement by yielding test implications all of which are consistent with the rival theory, then the hypothesis is simply "contained within" it and offers no new empirical information (Post, 1971, Ch. 3). Thus, the second eligibility requirement I propose is that any hypothesis viable for testing must be amenable to severe tests by implying test statements which background theories would tell us are probably false.

In addition to prohibiting hypotheses that are contained within background theories, this second eligibility requirement in effect rules out empirically empty hypotheses. Since empirically empty hypotheses are trivially true, they fail to clash empirically with any rival theory, as frequently illustrated by excessively vague hypotheses. Popper has shown that many "scientists" try to ward off refutations by proposing vague or unintelligible hypotheses, but at the cost of producing hypotheses that are trivally true in relation to accepted background knowledge as a result (1959, pp. 269–273). Consider, for example, the following rather simplistic hypothesis: (1) We all voluntarily act in ways that promote our own individual pleasure. This doctrine minimizes empirical conflict with background knowledge because of such a vague expression as "ways that promote our own pleasure." Some contend that an act promotes the agent's pleasure simply if the act is what the agent voluntarily performs. But on this reading (1) is reduced to utter triviality for it basically amounts to the following: (1′) We all voluntarily act in those ways in which we voluntarily act. Is it possible reasonably to deny that we voluntarily do the things that we voluntarily do? Since a falsifying instance to (1′) is impossible, it is immune to empirical refutation. It fails to clash with any hypothesis. (1′) is clearly a necessary truth, a tautology, rather than a contingent claim purporting to describe the world. This indicates that empirically empty hypotheses, such as (1′), are poor candidates to explain the world and should not be subject to testing, as Popper has convincingly shown.

I have proposed that two conditions determine which hypotheses are viable for testing: (a) a viable hypothesis must account for all phenomena explained by its rival theory, and (b) it must empirically clash with its rival. Both of these two conditions involve significant historical considerations, as both requirements appeal to the particular rival scientific theories at a certain time of discussion. Whether a theory meets these conditions depends on the particular rival theories at a particular time. In that sense the scientific status of a theory depends on its history.

Historical Dimensions of the Demarcation Criterion

According to the metacriterion given above, all scientific theories are by definition viable candidates for testing. Unscientific theories, on the other hand, are untestworthy. Given the two eligibility requirements proposed above, we can finally suggest a new demarcation criterion. Every scientific theory, by definition, must first match its rival's empirical successes by accounting for all phenomena that the rival theory explains, and second, must empirically clash with its rival by yielding inconsistent test implications.

As a dividing line between science and pseudoscience, this criterion relies heavily on the general competitive climate of discussion at a particular time,

for the classification of a certain theory as scientific depends on how it stands in relation to its rivals at a given historical period. A scientific theory must compare favorably with its rivals' explanatory power as well as convey at least some conflicting empirical content in relation to its rivals. On this view science is essentially a relational property between a theory in question and some alternative. Ptolemaic cosmology is scientific in relation to Aristotelian cosmology since it equaled the explanatory successes of its predecessor and because it clashed empirically with Aristotle's theory concerning the planetary motions of retrogression. By incorporating the Copernican vision of a sun-dominated universe, Kepler proposed a theory that matched the explanatory power of Ptolemaic cosmology but that clashed with Ptolemy's theory concerning the orbital path of Mars. Kepler's theory is then scientific in relation to Ptolemy's. Galileo's dynamics could account for all the known phenomena explained by Aristotelian theory, but also implied many surprising results. Similarly, the special theory of relativity is scientific in relation to Newtonian theory of space and time. In addition to equaling the empirical successes of Newtonian theory, the theory of relativity predicts some surprising claims concerning the motion of high-speed subatomic particles.

If a theory fails to meet one of the eligibility requirements, the theory is not only untestworthy but also unscientific. One class of unscientific theories includes those that cannot explain some phenomenon that is successfully explained by a rival. In relation to today's conventionally accepted theories, unscientific hypotheses include the theory of divine creation, mythical beliefs of witchcraft and spiritual possession, and various forms of medical quackery (Gardner, 1952). Astrology is also unscientific in relation to contemporary rivals, for astrology fails to explain many known events, such as the identical fate of many victims of some human tragedy. But some of these unscientific theories conceivably could have reflected the most advanced state of knowledge at a certain period in history, and thus at that time deserved the label of science relative to their rivals. In ancient Greece astrology was a viable theory for testing in relation to alternatives, allowing us to identify astrology as science at least in that early historical period.

A second class of unscientific theories are by definition unworthy of testing because they yield no surprising results in relation to their rival. Since these theories are empirically contained within the rival background theory, they yield no potential for empirical improvement, as illustrated by empirically empty theories. Many critics charge that Freudian psychoanalysis, for example, is empirically empty because of its ability to explain any conceivable behavior (Popper, 1962, pp. 34–36) and because of its inability to clash with any theory.

The difference between the above demarcation criterion and Popper's is that the criterion proposed here imposes more stringent requirements for

genuine science. The second eligibility requirement—that science must yield unexpected results—includes Popper's condition of falsifiability, since any hypothesis that yields surprising empirical consequences is falsifiable. But since falsifiability is an absolute property of a theory, Popper's criterion is thoroughly ahistorical, as is his classification of science and non-science. On my view science is primarily a relational property between a theory and some rival at a particular time.

Some critics with more formalist leanings may object that the historical character of this criterion yields counter-intuitive results. On this objection, the historical aspect of the criterion produces an unacceptable flexibility and uncertainty since the criterion allows for the possibility that some theories may be subject to a series of reclassifications in the future. The criterion permits Newtonian mechanics, for example, to be repeatedly reclassified: Newtonian mechanics is scientific in relation to its eighteenth-century rivals, but it is unscientific in relation to twentieth-century theories, but it may be scientific again in relation to some future theories, and so on. This intuitively unacceptable flexibility in the classification of science and non-science is the result of the view that science is primarily a relational concept between a theory and some rival at a particular time. The status of a theory should be immune to alteration by historical considerations, according to this objection.

But the force of this objection is at best rather weak, for our intuitions on this matter also seem to suggest that a historical demarcation criterion would be too restrictive by failing to allow for the reclassification of instances of science and non-science. Many domains in ancient Greece, for example, domains that today are called superstition, religion, magic, and the occult, were at that time clear cases of legitimate science (Crombie, 1953, pp. 5–7). But with the more stringent testing standards of modern science, these domains were removed from the rolls of science because they were immune to strict experimental appraisal (Crombie, 1953, p. 2). The advent of modern science brought with it a major reclassification of theories. The possibility of reclassifying theories in light of epistemological advances is an important intuitive feature for any demarcation criterion, one that an ahistorical criterion lacks. The point here is that our intuitive judgments on this issue are inconsistent—some intuitive considerations recommend a historical criterion whereas others recommend an ahistorical one. If this is the case, intuitions alone cannot determine which type of criterion, historical or ahistorical, is preferable. The objection that the criterion given above violates our intuitive leanings toward an ahistorical criterion is therefore unconvincing.

It should be noted that the proposed criterion does not define science on historical grounds alone, for some theories are classified as unscientific regardless of the historical climate at a given time. For example, a theory *t* that is unscientific at a given time because it fails to match the explanatory

success of its rival will be unscientific relative to any other theory replacing the rival. Again, if *t* fails to account for some event that is explained by a rival theory, then *t* will fail to explain this event relative to any future theory replacing the particular rival. The reason is that all future theories that replace the rival theory will match the earlier explanatory success, following the criterion. This, in effect, prohibits *t* from every being worthy of testing. This ahistorical status of theories such as *t* is attributable to their failure to exhibit enough relative promise to merit testing, following the criterion. Because these theories are untestworthy, as opposed to some rivals, theories such as *t* will retain their status as pseudoscience. Moreover, theories that convey no empirical import, that is, theories that are immune to experimental testing, are always in violation of the requirement that all science must empirically clash with some rival. As shown above, such theories fail to clash empirically with any theory, which, according to the eligibility requirements given above, means that they can never be viable candidates for testing. The indictment against these theories is not that they are all false, but instead that they are always immune to empirical and "scientific" appraisal.

Admittedly, the proposed demarcation criterion will no doubt fail to accommodate at least some clear cases of science or non-science. This raises the perplexing issue of balancing advantages and disadvantages of any conceptual proposal. To what degree, for example, should our judgments about unobjectionable cases be sacrificed for conceptual clarity and simplicity? Although these problems obviously surpass the scope of this paper, I believe nevertheless that the criterion presented above avoids any major systematic conflict with our judgments about clear cases, and is thus an improvement over Popper's criterion which is subject to such conflicts. Moreover, because of its historical emphasis, the criterion is realistically grounded in the actual epistemic struggle between rival theories, competing for supremacy in a preferential ordering. On this criterion it is viability for testing, a matter that changes with history, that divides genuine science from pseudoscience.

Notes

1. The word "science" has many fundamentally different meanings in ordinary use. "Science" refers to a "rational" methodology for acquiring knowledge, a particular branch of knowledge, or systematic knowledge of the world in general. But none of these meanings captures the notion of science that concerns us here. For my purpose, "science" refers to certain systems of statements, pardigm cases being Newtonian mechanics and contemporary astrophysics as opposed to pseudoscientific systems such as witchcraft and voodoo.

2. Popper defines a basic statement as a singular existential statement "asserting that an observable event is occurring in a certain individual region of space and time" (1959, p. 103).

3. Even though Popper acknowledges the pseudoscientific nature of astrology (1962, pp. 34, 53, 188), astrology is falsifiable, as evidenced by the repetition of

falsifying instances. These falsifications arise from mass human tragedies like wars and earthquakes, since the victims suffer the same tragic fate together despite divergent astrological horoscopes. Other refuting instances occur when two people, born simultaneously with identical celestial influences, experience different fates (Jerome, 1975).

4. No philosopher, to my knowledge, has proposed this metacriterion, and some philosophers offer demarcation criteria that do not conform to the metacriterion. One eligibility requirement for Popper, for example, is that a scientific theory must convey a high degree of empirical content. But Popper does not use this requirement as a defining feature of science, although he clearly prescribes it as one standard for successful science.

5. My proposal restricts how we should define science, and in doing so, amounts to the following metacriterion, *M*, for any adequate demarcation criterion:

> *M*: Any adequate demarcation criterion defines a genuinely scientific theory as a system of statements satisfying all eligibility requirements from our accepted methodological theory of appraisal.

Despite this strict relativity of the notion of science to an accepted methodology, *M* is totally neutral with respect to the selection of particular eligibility requirements. But once such requirements are selected, the demarcation between science and non-science follows quite easily.

6. Some commentators have attributed to Popper the view that absolute falsification is the primary function of testing (Ayer, 1952, p. 32). But Popper asserts that conclusive falsification is unattainable, since we can always "transfer responsibility for the empirical refutation to one or more of the cooperating hypotheses" (1959, p. 50).

7. H. R. Post is one advocate of this aim in science and he cites a number of scientists who share this view, including Max Born, P. Duhem, and J. Clerk Maxwell (Post, 1971, p. 213). As William Whewell wrote,

> The principles which constituted the triumph of the preceding stages of the science, may appear to be subverted and ejected by the later discoveries, but in fact they are (so far as they were true) taken up into the subsequent doctrines and included in them. They continue to be an essential part of the science. The earlier truths are not expelled but absorbed, not contradicted but extended; and the history of each science, which may thus appear like a succession of revolutions, is in reality, a series of developments. (1857, p. 8, cited by Post, 1971)

8. The explanadum is, by definition, the sentence describing the phenomenon to be explained. The explanans is, by definition, the set of sentences used to account for the phenomenon.

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Logic of Discovery or Psychology of Research?

THOMAS S. KUHN

Among the most fundamental issues on which Sir Karl and I agree is our insistence that an analysis of the development of scientific knowledge must take account of the way science has actually been practiced. That being so, a few of his recurrent generalizations startle me. One of these provides the opening sentences of the first chapter of the *Logic of Scientific Discovery:* "A scientist," writes Sir Karl, "whether theorist or experimenter, puts forward statements, or systems of statements, and tests them step by step. In the field of the empirical sciences, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment."[1] The statement is virtually a cliché, yet in application it presents three problems. It is ambiguous in its failure to specify which of two sorts of "statements" or "theories" are being tested. That ambiguity can, it is true, be eliminated by reference to other passages in Sir Karl's writings, but the generalization that results is historically mistaken. Furthermore, the mistake proves important, for the unambiguous form of the description misses just that characteristic of scientific practice which most nearly distinguishes the sciences from other creative pursuits.

There is one sort of "statement" or "hypothesis" that scientists do repeatedly subject to systematic test. I have in mind statements of an individual's best guesses about the proper way to connect his own research problem with the corpus of accepted scientific knowledge. He may, for example, conjecture that a given chemical unknown contains the salt of a rare earth, that the obesity of his experimental rats is due to a specified component in their diet, or that a newly discovered spectral pattern is to be understood as an effect of nuclear spin. In each case, the next steps in his research are

intended to try out or test the conjecture or hypothesis. If it passes enough or stringent enough tests, the scientist has made a discovery or has at least resolved the puzzle he had been set. If not, he must either abandon the puzzle entirely or attempt to solve it with the aid of some other hypothesis. Many research problems, though by no means all, take this form. Tests of this sort are a standard component of what I have elsewhere labelled "normal science" or "normal research," an enterprise which accounts for the overwhelming majority of the work done in basic science. In no usual sense, however, are such tests directed to current theory. On the contrary, when engaged with a normal research problem, the scientist must *premise* current theory as the rules of his game. His object is to solve a puzzle, preferably one at which others have failed, and current theory is required to define that puzzle and to guarantee that, given sufficient brilliance, it can be solved.[2] Of course the practitioner of such an enterprise must often test the conjectural puzzle solution that his ingenuity suggests. But only his personal conjecture is tested. If it fails the test, only his own ability not the corpus of current science is impugned. In short, though tests occur frequently in normal science, these tests are of a peculiar sort, for in the final analysis it is the individual scientist rather than current theory which is tested.

This is not, however, the sort of test Sir Karl has in mind. He is above all concerned with the procedures through which science grows, and he is convinced that "growth" occurs not primarily by accretion but by the revolutionary overthrow of an accepted theory and its replacement by a better one.[3] (The subsumption under "growth" of "repeated overthrow" is itself a linguistic oddity whose *raison d'être* may become more visible as we proceed.) Taking this view, the tests which Sir Karl emphasizes are those which were performed to explore the limitations of accepted theory or to subject a current theory to maximum strain. Among his favorite examples, all of them startling and destructive in their outcome, are Lavoisier's experiments on calcination, the eclipse expedition of 1919, and the recent experiments on parity conservation.[4] All, of course, are classic tests, but in using them to characterize scientific activity Sir Karl misses something terribly important about them. Episodes like these are very rare in the development of science. When they occur, they are generally called forth either by a prior crisis in the relevant field (Lavoisier's experiments or Lee and Yang's[5]) or by the existence of a theory which competes with the existing canons of research (Einstein's general relativity). These are, however, aspects of or occasions for what I have elsewhere called "extraordinary research," an enterprise in which scientists do display very many of the characteristics Sir Karl emphasizes, but one which, at least in the past, has arisen only intermittently and under quite special circumstances in any scientific speciality.[6]

I suggest then that Sir Karl has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts. His

emphasis is natural and common: the exploits of a Copernicus or Einstein make better reading than those of a Brahe or Lorentz; Sir Karl would not be the first if he mistook what I call normal science for an intrinsically uninteresting enterprise. Nevertheless, neither science nor the development of knowledge is likely to be understood if research is viewed exclusively through the revolutions it occasionally produces. For example, though testing of basic commitments occurs only in extraordinary science, it is normal science that discloses both the points to test and the manner of testing. Or again, it is for the normal, not the extraordinary practice of science that professionals are trained; if they are nevertheless eminently successful in displacing and replacing the theories on which normal practice depends, that is an oddity which must be explained. Finally, and this is for now my main point, a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's sort of testing does not occur, rather than extraordinary science which most nearly distinguishes science from other enterprises. If a demarcation criterion exists (we must not, I think, seek a sharp or decisive one), it may lie just in that part of science which Sir Karl ignores.

In one of his most evocative essays, Sir Karl traces the origin of "the tradition of critical discussion [which] represents the only practicable way of expanding our knowledge" to the Greek philosophers between Thales and Plato, the men who, as he sees it, encouraged critical discussion both between schools and within individual schools.[7] The accompanying description of Presocratic discourse is most apt, but what is described does not at all resemble science. Rather it is the tradition of claims, counter-claims, and debates over fundamentals which, except perhaps during the Middle Ages, have characterized philosophy and much of social science ever since. Already by the Hellenistic period mathematics, astronomy, statics and the geometric parts of optics had abandoned this mode of discourse in favour of puzzle solving. Other sciences, in increasing numbers, have undergone the same transition since. In a sense, to turn Sir Karl's view on its head, it is precisely the abandonment of critical discourse that marks the transition to a science. Once a field has made that transition, critical discourse recurs only at moments of crisis when the bases of the field are again in jeopardy.[8] Only when they must choose between competing theories do scientists behave like philosophers. That, I think, is why Sir Karl's brilliant description of the reasons for the choice between metaphysical systems so closely resembles my description of the reasons for choosing between scientific theories.[9] In neither choice, as I shall shortly try to show, can testing play a quite decisive role.

There is, however, good reason why testing has seemed to do so, and in exploring it Sir Karl's duck may at last become my rabbit. No puzzle-solving enterprise can exist unless its practitioners share criteria which, for that group and for that time, determine when a particular puzzle has been solved. The

same criteria necessarily determine failure to achieve a solution, and anyone who chooses may view that failure as the failure of a theory to pass a test. Normally, as I have already insisted, it is not viewed that way. Only the practitioner is blamed, not his tools. But under the special circumstances which induce a crisis in the profession (e.g. gross failure, or repeated failure by the most brilliant professionals) the group's opinion may change. A failure that had previously been personal may then come to seem the failure of a theory under test. Thereafter, because the test arose from a puzzle and thus carried settled criteria of solution, it proves both more severe and harder to evade than the tests available within a tradition whose normal mode is critical discourse rather than puzzle solving.

In a sense, therefore, severity of test-criteria is simply one side of the coin whose other face is a puzzle-solving tradition. That is why Sir Karl's line of demarcation and my own so frequently coincide. That coincidence is, however, only in their *outcome*; the *process* of applying them is very different, and it isolates distinct aspects of the activity about which the decision—science or non-science—is to be made. Examining the vexing cases, for example, psychoanalysis or Marxist historiography, for which Sir Karl tells us his criterion was initially designed,[10] I concur that they cannot now properly be labelled "science." But I reach that conclusion by a route far surer and more direct than his. One brief example may suggest that of the two criteria, testing and puzzle solving, the latter is at once the less equivocal and the more fundamental.

To avoid irrelevant contemporary controversies, I consider astrology rather than, say, psychoanalysis. Astrology is Sir Karl's most frequently cited example of a "pseudo-science."[11] He says: "By making their interpretations and prophecies sufficiently vague they [astrologers] were able to explain away anything that might have been a refutation of the theory had the theory and the prophecies been more precise. In order to escape falsification they destroyed the testability of the theory."[12] Those generalizations catch something of the spirit of the astrological enterprise. But taken at all literally, as they must be if they are to provide a demarcation criterion, they are impossible to support. The history of astrology during the centuries when it was intellectually reputable records many predictions that categorically failed.[13] Not even astrology's most convinced and vehement exponents doubted the recurrence of such failures. Astrology cannot be barred from the sciences because of the form in which its predictions were cast.

Nor can it be barred because of the way its practitioners explained failure. Astrologers pointed out, for example, that, unlike general predictions about, say, an individual's propensities or a natural calamity, the forecast of an individual's future was an immensely complex task, demanding the utmost skill, and extremely sensitive to minor errors in relevant data. The configuration of the stars and eight planets was constantly changing; the astro-

nomical tables used to compute the configuration at an individual's birth were notoriously imperfect; few men knew the instant of their birth with the requisite precision.[14] No wonder, then, that forecasts often failed. Only after astrology itself became implausible did these arguments come to seem question-begging.[15] Similar arguments are regularly used today when explaining, for example, failures in medicine or meteorology. In times of trouble they are also deployed in the exact sciences, fields like physics, chemistry, and astronomy.[16] There was nothing unscientific about the astrologer's explanation of failure.

Nevertheless, astrology was not a science. Instead it was a craft, one of the practical arts, with close resemblances to engineering, meteorology, and medicine as these fields were practised until little more than a century ago. The parallels to an older medicine and to contemporary psychoanalysis are, I think, particularly close. In each of these fields shared theory was adequate only to establish the plausibility of the discipline and to provide a rationale for the various craft-rules which governed practice. These rules had proved their use in the past, but no practitioner supposed they were sufficient to prevent recurrent failure. A more articulated theory and more powerful rules were desired, but it would have been absurd to abandon a plausible and badly needed discipline with a tradition of limited success simply because these desiderata were not yet at hand. In their absence, however, neither the astrologer nor the doctor could do research. Though they had rules to apply, they had no puzzles to solve and therefore no science to practise.[17]

Compare the situations of the astronomer and the astrologer. If an astronomer's prediction failed and his calculations checked, he could hope to set the situation right. Perhaps the data were at fault; old observations could be re-examined and new measurements made, tasks which posed a host of calculational and instrumental puzzles. Or perhaps theory needed adjustment, either by the manipulation of epicycles, eccentrics, equants, etc., or by more fundamental reforms of astronomical technique. For more than a millennium these were the theoretical and mathematical puzzles around which, together with their instrumental counterparts, the astronomical research tradition was constituted. The astrologer, by contrast, had no such puzzles. The occurrence of failures could be explained, but particular failures did not give rise to research puzzles, for no man, however skilled, could make use of them in a constructive attempt to revise the astrological tradition. There were too many possible sources of difficulty, most of them beyond the astrologer's knowledge, control, or responsibility. Individual failures were correspondingly uninformative, and they did not reflect on the competence of the prognosticator in the eyes of his professional compeers.[18] Though astronomy and astrology were regularly practised by the same people, including Ptolemy, Kepler, and Tycho Brahe, there was never an astrological equivalent of the puzzle-solving astronomical tradition. And without puzzles,

able first to challenge and then to attest the ingenuity of the individual practioner, astrology could not have become a science even if the stars had, in fact, controlled human destiny.

In short, though astrologers made testable predictions and recognized that these predictions sometimes failed, they did not and could not engage in the sorts of activities that normally characterize all recognized sciences. Sir Karl is right to exclude astrology from the sciences, but his over-concentration on science's occasional revolutions prevents his seeing the surest reason for doing so.

That fact, in turn, may explain another oddity of Sir Karl's historiography. Though he repeatedly underlines the role of tests in the replacement of scientific theories, he is also constrained to recognize that many theories, for example the Ptolemaic, were replaced before they had in fact been tested.[19] On some occasions, at least, tests are not requisite to the revolutions through which science advances. But that is not true of puzzles. Though the theories Sir Karl cites had not been put to the test before their displacement, none of these was replaced before it had ceased adequately to support a puzzle-solving tradition. The state of astronomy was a scandal in the early sixteenth century. Most astronomers nevertheless felt that normal adjustments of a basically Ptolemaic model would set the situation right. In this sense the theory had not failed a test. But a few astronomers, Copernicus among them, felt that the difficulties must lie in the Ptolemaic approach itself rather than in the particular versions of Ptolemaic theory so far developed, and the results of that conviction are already recorded. The stituation is typical.[20] With or without tests, a puzzle-solving tradition can prepare the way for its own displacement. To rely on testing as the mark of a science is to miss what scientists mostly do and, with it, the most characteristic feature of their enterprise.

Notes

1. Popper [1959], p. 27.
2. For an extended discussion of normal science, the activity which practitioners are trained to carry on, see my [1962], pp. 23–42, and 135–42. It is important to notice that when I describe the scientist as a puzzle solver and Sir Karl describes him as a problem solver (e.g. in his [1963], pp. 67, 222), the similarity of our terms disguises a fundamental divergence. Sir Karl writes (the italics are his), "Admittedly, our expectations, and thus our theories, may precede, historically, even our problems. *Yet science starts only with problems*. Problems crop up especially when we are disappointed in our expectations, or when our theories involve us in difficulties, in contradictions." I use the term "puzzle" in order to emphasize that the difficulties which *ordinarily* confront even the very best scientists are, like crossword puzzles or chess puzzles, challenges only to his ingenuity. *He* is in difficulty, not current theory. My point is almost the converse of Sir Karl's.
3. Cf. Popper [1963], pp. 129, 215 and 221, for particularly forceful statements of this position.

4. For example, Popper [1963], p. 220.

5. For the work on calcination see, Guerlac [1961]. For the background of the parity experiments see, Hafner and Presswood [1965].

6. The point is argued at length in my [1962], pp. 52–97.

7. Popper [1963], chapter 5, especially pp. 148–52.

8. Though I was not then seeking a demarcation criterion, just these points are argued at length in my [1962], pp. 10–22 and 87–90.

9. Cf. Popper [1963], pp. 192–200, with my [1962], pp. 143–58.

10. Popper [1963], p. 34.

11. The index to Popper [1963] has eight entries under the heading "astrology as a typical pseudoscience."

12. Popper [1963], p. 37.

13. For examples see, Thorndike [1923–58], 5, pp. 225 ff.; 6, pp. 71, 101, 114.

14. For reiterated explanations of failure see, *ibid. x*, pp. 11 and 514ff.; 4, 368; 5, 279.

15. A perceptive account of some reasons for astrology's loss of plausibility is included in Stahlman [1956]. For an explanation of astrology's previous appeal see, Thorndike [1955].

16. Cf. my [1962], pp. 66–76.

17. This formulation suggests that Sir Karl's criterion of demarcation might be saved by a minor restatement entirely in keeping with his apparent intent. For a field to be a science its conclusions must be *logically derivable* from *shared premises*. On this view astrology is to be barred not because its forecasts were not testable but because only the most general and least testable ones could be derived from accepted theory. Since any field that did satisfy this condition *might* support a puzzle solving tradition, the suggestion is clearly helpful. It comes close to supplying a sufficient condition for a field's being a science. But in this form, at least, it is not even quite a sufficient condition, and it is surely not a necessary one. It would, for example, admit surveying and navigation as sciences, and it would bar taxonomy, historical geology, and the theory of evolution. The conclusions of a science may be both precise and binding without being fully derivable by logic from accepted premises. Cf. my [1962], pp. 35–51, and also the discussion in Section III, *below*.

18. This is not to suggest that astrologers did not criticize each other. On the contrary, like practioners of philosophy and some social sciences, they belonged to a variety of different schools, and the inter-school strife was sometimes bitter. But these debates ordinarily revolved about the *implausibility* of a particular theory employed by one or another school. Failures of individual predictions played very little role. Compare Thorndike [1923–58], 5, p. 233.

19. Cf. Popper [1963], p. 246.

20. Cf. my [1962], pp. 77–87.

References

Guerlac [1961]: *Lavoisier—The Crucial Year*, 1961.
Hafner and Presswood [1965]: "Strong Interference and Weak Interactions," *Science*, 149, pp. 503–10.
Kuhn [1962]: *The Structure of Scientific Revolutions*, 1962.
Popper [1959]: *Logic of Scientific Discovery*, 1959.
Popper [1963]: *Conjectures and Refutations*, 1963.

Stahlman [1956]: "Astrology in Colonial America: An Extended Query," *William and Mary Quarterly*, 13, pp. 551–63.
Thorndike [1923–58]: *A History of Magic and Experimental Science*, 8 vols, 1923–58.
Thorndike [1955]: "The True Place of Astrology in the History of Science," *Isis*, 46, pp. 273–8.

Kuhn, Popper, and the Normative Problem of Demarcation

ROBERT FELEPPA

Popper is not merely trying to distinguish science from pseudoscience. He is also making an important *evaluational* claim—a ranking of one as *better* than the other. Also, his demarcation criterion is meant to indicate scientific progress and improvement, and to establish science as *rational* in ways that pseudoscience is not. Ideally, adequate criteria of demarcation will prescriptively guide scientific behavior, rather than simply describe it, and will also sharpen our understanding of what it is to be rational.

Given all this, the task Popper sets is not then merely to identify criteria of demarcation, but to justify their evaluative force.

Popper's account of demarcation of course hinges centrally on his falsifiability criterion, and he has been widely criticized by Kuhn and others precisely for stressing this feature. In its place, Kuhn offers a cluster of criteria culled from what he takes to be a more sensitive reading of the historical record of science and from current scientific practice. But on the face of it, simply describing what scientists do seems to fall short of what is needed: the question of why they *ought* to act as they do seems unanswered. As many critics have pointed out, Kuhn's basis for inferring scientific and rational prescriptions on the basis of his historical descriptions is unclear: describing the genesis of a set of conventions does not alone serve to justify them.

How is the dispute between Popper and Kuhn to be resolved? Given that demarcation is fundamentally a normative issue, such resolution demands first a clearer understanding of the strategy each side employs in deriving prescriptive criteria from scientific practice. The debate goes much deeper than simply the question of whether one side or the other is correctly *char-*

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acterizing scientific practice. The different conclusions they draw from substantially the same historical record suggest that they may well disagree on some basic points concerning the proper way to derive a methodological "ought" from a historical "is." But Kuhn and Popper both take justification roughly to involve a movement from the description of the practice to be justified to the development of justifying principles. This suggests a strong compatibility between their implicit views on justification and the "reflective equilibrium" account of justification expounded by Nelson Goodman and John Rawls. In what follows I hope to clarify the normative aspects of the Popper–Kuhn controversy, and to work toward a more fruitful comparison of their positions, by appealing to this notion.

In Defense of Popper

In the preceding essay in this collection Kuhn uses Popper's often-cited case of astrology to explicate Popper's central contention that essential to proper scientific procedure is the subjectability of theory to experimental tests, which are, in Kuhn's words, "performed to explore the limitations of accepted theory or to subject a current theory to maximum strain." A scientific theory is better insofar as it risks more, and a "theory" that risks nothing, that is, one so constructed as to be immune to falsification, is not really an empirical theory at all. Popper deems astrology a pseudoscience because astrological systems are too easily made immune to empirical failure, either by explaining away predictive failure by citing unaccounted or unknown variables or through the use of language so vague as to allow easy reinterpretation of predictions to make them agree with subsequent events. Thus "saving the appearances" works to the discredit of astrological systems; on Popper's view, they fail to meet what he intends as a central *necessary* condition for being genuinely scientific. According to a relatively recent formulation (1963) p. 256:

> a system [1] is to be considered scientific only if it makes assertions which may clash with observations; and a system is, in fact, tested by attempts to refute it. Thus testability is the same as refutability, and can therefore likewise be taken as a criterion of demarcation. [2]

Kuhn's main objection to this, in the preceding essay, is that Popper's criterion is derived only from certain episodic—and atypical—fragments from the history of science, and thus presents a distorted account of scientific practice (keep in mind that ultimately this charge is leveled against Popper's justificatory account):

> a careful look at the scientific enterprise suggests that it is normal science, in which Sir Karl's sort of testing does not occur, rather than extraordinary science which most nearly distinguishes science from other enterprises. If a demar-

cation criterion exists (we must not, I think, seek a sharp or decisive one), it may lie just in that part of science which Sir Karl ignores.

As he indicates in this passage, Kuhn looks instead to the practice of normal science in its periods of relatively crisis-free development in order to discover criteria of demarcation, contrasting astronomy with astrology in terms of the former's greater articulation and more powerful governing rules (as opposed to the latter's weaker "craft-rules").

Kuhn also questions Popper's account of the pseudoscientific character of astrology on the grounds that astrological practice actually quite often conforms to Popper's criterion. Although he agrees that Popper's focus on the untestability of astrology "catches something of the spirit of the astrological enterprise," Kuhn rejects Popper's criterion because "the history of astrology during the centuries when it was intellectually reputable records many predictions that categorically failed. Not even astrology's most convinced and vehement exponents doubted the recurrence of such failures."

But clearly Popper has a legitimate reply to Kuhn here on a number of counts. First, as I emphasized above, Popper should not be too quickly interpreted as offering a *sufficient* condition for demarcating science. The above passage indicates that it is to be taken only as a necessary condition, and hence Kuhn's insistence that a pseudoscience in historical fact is refutable on occasion does not speak directly to Popper's point.[3]

Also, even were this pertinent (Popper has on occasion suggested a stronger logical status for this criterion), why should the *occasional* adherence of astrologers to the falsifiability criterion serve to refute his contention? Is he not demanding unfailing, or at least general majority acceptance of this criterion within the relevant communities? And surely astrologers have been and still are guilty of the sort of exploitation of indeterminacy and vagueness that Popper decries.

This might lead us to ask why Kuhn cites the *earlier* history of astrology—when it was not only more reputable, as Kuhn remarks, but also far less easily distinguishable from astronomy in the eyes of its practitioners at the time as well as of subsequent historians. If Popper is to be refuted here, it would seem preferable to examine more recent astrological strains that are more easily distinguishable from what we take to be genuine science. Otherwise Popper could cite just this very intermixing of legitimate and metaphysical strands to explain why the astrologer-astronomer's behavior happened to conform more closely to his proposed standard in earlier times.

Moreover, Kuhn makes no mention of the replacement or revision of any astrological system as the result of empirical failure—and it is these sorts of consequences in which Popper is primarily interested: he wants more than the *admission* of error. His view is that scientists *learn* from their mistakes in the sense of using them to generate new theoretical products. (In this regard

he could aptly cite Kuhn's remark that no astrologer "could make use of [research puzzles] in a constructive attempt to revise the astrological tradition.")

However, there is a deeper puzzle behind these various objections concerning just how we are to learn from the historical record what scientists, and rational persons generally, ought to be doing. The gist of Kuhn's criticisms seems simply to be that scientists do not behave in the way Popper claims they should (or that nonscientists do behave this way at times). He criticizes Popper for drawing on insufficient historical data and thus giving a distorted account of what they do *and* of what they ought to do. But should we not expect that a normative account will deviate from actual practice to some degree? Simply showing that Popper is appealing only to certain features of the historical record—which *exemplify* his chosen standard—does not prove that he is *distorting* things; we must first look more carefully at Popper's reasons for selecting as he does. Showing that one or the other account is "truer" to the historical record is beside the point where the primary question is one of justification: normative criteria are not the mere reflection of actual practice.

The cogency of Kuhn's objections is further clouded by the fact that it is not clear that Popper *has* ignored normal science. In specific rejoinder to the criticisms I have discussed, Popper (Lakatos and Musgrave, p. 51) points out that he noted the fundamental role of normal practice as a background for progressive change (through conjecture and refutation) as early as the writing of the opening paragraph of *The Logic of Scientific Discovery*:

> A scientist engaged in a piece of research . . . can attack his problem straight away. He can go at once to the heart of an organized structure. For a structure of scientific doctrines is already in existence; and with it, a generally accepted problem-situation. This is why he may leave it to others to fit his contribution into the framework of scientific knowledge.

Popper does not wish so much to discredit or ignore normal science as to uncover what it is that makes for *good* science, normal or abnormal. And if one considers that it is in reaction to some widely current forms of normal Freudian psychology and Marxist historiography that Popper often writes, it becomes clearer that simply citing deviations of normal practice from Popper's prescriptions is insufficient.

Popper gives a lot of attention to extraordinary science, but he does not thus mean to *ignore* normal practice. Instead, he intends to determine *good* normal practice in virtue of its *continuity* with extraordinary science. Adherence to the falsifiability criterion is intended to ensure that normal, nonrevolutionary practice never becomes so structured as to preclude the possibility of empirical refutation or theoretical revolution. Whatever lengths a scientist may go to protect a theory, Popper desires that these defenses

never attain perfect impregnability to recalcitrant experience. So, for instance, Kuhn's remarks concerning the similar appeal made by meteorologists and astrologers to unknown and complex variables do not present any immediate threat to Popper's demaracation of those fields: what is important for him is not *that* such a theory-saving strategy is used, but instead the degree to which it is used.

In Defense of Kuhn: A Reconstruction

To thus cite normal practice so uncritically in responding to Popper is to miss, then, his most central point. It is for this reason that Popper (Lakatos and Musgrave, p. 52) is so harshly critical of Kuhn in this regard:

> "Normal" science, in Kuhn's sense, exists. It is the activity of the non-revolutionary, or more precisely, the not-too-critical professional: of the student who accepts the ruling dogma of the day; who does not wish to challenge it; and who accepts a new revolutionary theory only if almost everybody else is ready to accept it—if it becomes fashionable by a kind of bandwagon effect. To resist a new fashion needs perhaps as much courage as was needed to bring it about.

Popper and others contend that Kuhn has offered nothing by way of an independent and critical standpoint for justifying what scientists do—and yet, in Popper's view, this is precisely what is needed if one is justifiably to judge science as progressing rationally or as improving over time. Kuhn, in answering charges that he has made science irrational, cites a changing but reasonably stable list of criteria by which to judge the genuine progress of science. Despite substantive theoretical disagreements, scientists generally agree that competing theories are to be judged in terms of their generality in scope, simplicity, accuracy, consistency, fruitfulness, and so on; and, furthermore, Kuhn contends that these criteria provide an objective measure of scientific progress (Imre and Lakatos, p. 264; Kuhn, 1977, pp. 321ff.). But merely citing the fact that professional agreement on these criteria spans disagreements over theoretical content seems insufficient to justify their normative status. It does not seem enough to say that these criteria are those used by members of the scientific community in rating theories and measuring progress—not if one is not willing simply to accept what scientists do as correct but is instead trying to *evaluate* what they do and to *justify* certain procedures and label others as "unscientific" (though they might comprise the common practice of certain scientists). Since people do not always do what they ought to do, a "justification" that consists of making norms conform to practice seems vacuous and circular (perhaps, Popper might contend, manifesting the "bandwagon" effect again at another level).

As Carl Hempel (1981) has recently noted the problem here is not necessarily that Kuhn's strategy is misguided—it is just that certain key

justificatory steps are not made evident. Although he is generally in agreement with Kuhn's position (this has not always been the case), Hempel contends that Kuhn's arguments to establish the rationality of science do not go far enough. They show only that since scientists ought to behave as if their concern was to improve scientific knowledge, they ought then to adhere to their generally accepted criteria of theory-choice, since no other mode of behavior seems to serve this basic purpose as well. However, Hempel (p. 402) argues, aside from the fact that Kuhn has neither clarified what he takes the essential characteristics of science to be nor given an adequate account of what is to count as "improving scientific knowledge," we are left by this functional account with a glaring gap in normative reasoning:

> on Kuhn's view, scientific theory choice (and other facets of scientific behavior) are not effected by means of procedures that are deliberately adopted by the scientific community as a presumably optimal means for advancing scientific knowledge; and I would think that any kind of action, including scientific theory choice, can be called *rational* only if it can be causally linked to deliberation or reasoning aimed at achieving specific ends. Scientific theory choice as characterized by Kuhn would not be rational in this sense, but would rather be akin to what in anthropology are called *latently functional* behavior patterns, which serve a function they were never *chosen* to fulfill.

Hempel's contention is that the evaluation of the rationality of scientific theory-choice must be made in the light of general norms of rationality. And, in particular, that whatever the other virtues of the Kuhnian account, it cannot succeed in establishing the rationality of scientific practice without at least making explicit the connection between the adoption of choice-criteria and deliberation about how these criteria are to serve the basic purposes of science.

Hempel's criticism here is deep, but it nevertheless manifests a fundamental agreement with Kuhn's "historic-pragmatist" approach to science. For Hempel's criticism appeals to a norm drawn from our intuitive background of norms regarding rationality, and he does not claim to give it any more transcendental or a priori status than that. He agrees that norms of rational and scientific behavior are derived from our common practices wherein such norms operate. His only objection is that Kuhn does not seem to take into account all the pertinent norms in giving his justificatory account of science.

Hempel believes that the crucial connection between the evolution of choice-criteria and deliberation about practice is established in a pragmatist account of justification developed by Nelson Goodman in addressing the problem of induction in *Fact, Fiction, and Forecast*. Furthermore, he contends, Goodman's view can easily be adapted to the justificatory context that concerns Kuhn. Hempel argues that where the rules of choice can be made sufficiently explicit,[4] their justification can be achieved not by finding in-

dubitable first principles, but by realizing with Goodman that justification (in any context) consists not in giving an a priori grounding of the practice in question, but rather in *codifying* that practice. In Goodman's view, we justify our inductive (and, for that matter, our deductive) inferences by realizing from the start that we *do* make *some* valid inferences, prior to any full realization of principles that differentiate valid from invalid inferences. Just as Aristotle did in the *Organon*, we work initially from a set of inferences that we take to be clearly valid and a set that we take to be clearly invalid, and we endeavor to find principles that will succeed in generating the valid ones and in ruling out the invalid ones: the main objective being to improve our ability to assess inductive or deductive validity through this explicit formulation of principles. "Justification," in this modified sense, is achieved by giving more precise formulation to the principles we already use to assess our practice, and by seeking gradually to attain a suitable fit between those rules and the practice. Rawls, in adapting this notion of justification to ethical contexts in *A Theory of Justice*, has called this intended balance between principles and practice "reflective equilibrium." The principles "govern" the practice in that they are, after codification, appealed to in order to make determinations of validity or invalidity, and so on, and in that they provide certain advantages over the uncodified practice such as enabling one to judge what before were indeterminate "borderline" cases. However, it is important to emphasize that on the Goodman-Rawls account these principles are not regarded as fixed Platonic absolutes derived from careful reasoned consideration of the flux of scientific practice (cf. my discussion of Popper below). Instead, these principles are always in an important way answerable to the intuitive, pre-systematic judgments from which they were derived. Where these "governing" principles conflict overtly with the stronger of these intuitive judgments (i.e., judgments made without recourse to the principles), the principles themselves may have to be modified.

We operate, then, in Goodman's (1973, p. 14) view, in a virtuous circle:

> The point is that rules and particular inferences alike are justified by being brought into agreement with each other. A rule is amended if it yields an inference we are unwilling to amend. The process of justification is the delicate one of making mutual adjustments between rules and accepted inferences; and in the agreement achieved lies the only justification needed for either. [6]

Since Kuhn is concerned with a problem of justification that is very similar in character to Goodman's—namely, one of hypothesis- and theory-acceptance in science—and since Kuhn and Goodman share a similar pragmatist orientation, Hempel believes that Goodman's view of justification can serve to supplement Kuhn's argument. This may be possible without forcing any significant modification in what Kuhn does say. We now see how a justification of a practice *can* be developed without vicious circularity: we find

what sort of codification of principles will improve on the existing conventions, and realize that the justification we seek lies in our achieving improvements without incurring serious violations of our intuitive practice of theory-evaluation, theory-choice, and so on. Although this sort of reconstrual of such a fundamental notion as "justification" may strike some as "having all the advantages of theft over honest toil," it is an appealing solution to a number of age-old normative problems—the main point being that we should come to realize that all the troubles we have had in actually nailing down the justification of induction, a mode of inference we are sure is legitimate and justifiable in many instances, stem not from lack of ingenuity but from a deeper problem: namely, a miscast a prioristic attitude toward the nature of our task. And the solution to the problem lies precisely in accepting the fact that our presystematic judgments are to some degree legitimate, and that justification is achieved by moving, via codification, from an already existing set of values to a better one. We do not simply infer what we ought to do from what we do, but instead develop more general principles, and a systematically more cohesive understanding, concerning what is taken to be an already partially viable process of valuation.

Popper and Reflective Equilibrium

Thus Kuhn's position might be saved from the attacks of Popper and others by an appeal to a justificatory notion of reflective equilibrium. But might not Popper's position also be characterized in terms of reflective equilibrium? If so, we would have a firm basis of comparison—something evidently lacking in the controversy as initially surveyed above.

It might seem that the core idea of reflective equilibrium justification—to get governing principles without determining absolute essences—is fundamentally at odds with what Popper is trying to do. Popper could easily be read as offering falsifiability as a basic, transcendent feature of empirical science to which all practice must conform. This might well seem the case given the attention Popper directs to this criterion, its centrality to his account, and the vigor (some would use a less complimentary expression) with which he has defended it.

But there are a number of passages in Popper's general discussion of falsifiability in *The Logic of Scientific Discovery* that reveal, I think, certain important affinities with Goodman's views on justification (and hence with the Hempel-Kuhn account). Whatever Popper's actual intentions, these affinities suggest a reconstruction of his position along reflective equilibrium lines. First, Popper strongly emphasizes that his criterion of demarcation must "be regarded as a proposal for an agreement or convention" among "parties having some purpose in common." Furthermore, he admits that his derivation of justificatory principles for science has "been guided, in the last

analysis, by value judgments and predilections" (1959, p. 37). However, in light of these points he contends that he must then abandon the hope of giving any a priori grounding to his principles or of "representing them as the true or the essential aims of science"—they can be justified only through analyzing their logical consequences and determining "their fertility [and] their power to elucidate the problems of the theory of knowledge" (p. 38). Thus:

> My only reason for proposing my criterion of demarcation is that it is fruitful: that a great many points can be clarified and explained with its help. . . . It is only from the consequences of my definition of empirical science, and from the methodological decisions which depend upon this definition, that the scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavors. (p. 55)

In addition to giving this general account of his strategy, Popper attempts to give his account a clear structure, one that sets up a "first principle"—the falsifiability criterion—to generate methodological consequences that are to be examined in light of the intuitive judgments of scientists regarding both the basic aims of science and the presystematically adopted norms of theory- and hypothesis-acceptance. These "lower-order" norms are just the criteria of simplicity, scope, accuracy, and so on that are taken to function with some degree of legitimacy in science (p. 54).

Having thus outlined the strategy and structure of his program, Popper devotes subsequent sections of *The Logic of Scientific Discovery* to discussions of how the falsifiability criterion generates these standard criteria of adequacy. So, for instance, he argues that a theory that makes more precise predictions increases the number of experimental values that can clash with it, thus increasing its falsifiability, and is thus justifiably to be valued. Analogous arguments are offered for generality, simplicity, and so on.[7] In addition to these case judgments regarding the relative merits of legitimate theories, there are judgments of scientific legitimacy made throughout Popper's work that are to be measured by reference to the presystematic background. Hence the falsifiability criterion should not serve to rule out, say, quantum chromodynamics as nonempirical metaphysics.

Popper also contends that this criterion should provide an improved formulation of our already existing and evolving concept of science—that is, the set of value judgments commonly accepted regarding the basic aims and purposes of science. Among the values cited in Popper's work are the following: New theories should represent genuinely new discoveries that enable a deeper understanding about the world. Science should probe progressively deeper into the structure of the universe. Scientists should eschew dogmatist tendencies. Theories should have rich content and should reveal maximal "coherence," "compactness," and "organicity" in the states of

affairs they describe. Theories should have heuristic power and should provide guidance and stimulation for our imagination and intuition.[8]

By insisting that theorists adhere to the falsifiability criterion, Popper hopes to assure compliance to these general background norms, while clarifying precisely what constitutes such compliance. Thus, for instance, he contends that the progressively greater depth of advancing theories is ensured by the reduction and correction of earlier, refutable theories by more comprehensive successors; and their "world-aboutness" is guaranteed by assuring that they are empirically refutable.

Under the present reconstruction, then, the falsifiability criterion is to be "tested" on the basis of (1) whether it generates rules of practice, and, derivatively, evaluations of theories or parts of theories as better or worse, genuinely empirical or metaphysical (or utterly pseudoscientific), that accord with our presystematic judgments on these matters, and (2) whether it enables a reformulation of our roughly formulated background values that seems to improve our understanding of this background and to clarify its relationship to practice.

I think we can see at least the roots of a reflective-equilibrium account in these remarks of Popper's. Superficial appearances notwithstanding, the justificational approaches of Kuhn and Popper can thus be seen to share many important features.

Of course important differences remain. For, as noted above, Popper's program has a specifically elaborated structure—one that establishes a prior (though not an a priori) principle, a set of tasks for it to achieve, and a general account of how to determine the acceptability of this and other scientific norms in the light of prior practice.[9] The Hempel–Kuhn account, on the other hand, seems only to manifest the last, general feature.

Indeed, under this reconstruction, Popper's justificational strategy can be characterized in the same terms that he uses to characterize science—namely, as guided by an attitude of "modified essentialism" (1972, p. 202). Just as on Popper's account, scientists should proceed *as if* they were out after *the* true account of the "deep structure" of nature, so should the metascientist proceed as if there are essential features of science to be revealed in as systematic a way as possible—ideally in terms of one or several basic characteristics such as falsifiability: yet in the full light of a strong skepticism with regard to any attempt to "absolutize" these results. Popper's reasons for thus clearly structuring his metascientific program concern more the maintenance of a clear critical standpoint rather than the revelation of true essences. It is this feature of Platonic essentialism that he most highly values and that he endeavors to infuse into science and metascience. Popper wishes to critically assess and understand scientific practices and norms rather than simply to accept them. He regards his justification of such norms as value precision and simplicity in science, in terms of maximization of falsifiability,

as distinctly superior to the mere acceptance of received scientific opinion (1972, p. 204). In the Platonic critical spirit, he would rather honor scientists because he believes they are right, as opposed to the other way around.

The Relative Merits of Popper and Kuhn

We have seen that both Kuhn's and Popper's positions can be augmented and supplemented by placing them in the pragmatic normative setting of reflective equilibrium. Such a construction is certainly not alien to either position, since both are evolutionary and non–a priori in outlook, and since each is firmly committed to a justificatory strategy that derives prescriptions from preexisting conventions.

But it is clear that there is a difference in the structuring of the programs—or, instead, a difference between a clear structure in one case and *no* evident structure in the other. The value of Popper's structuring may be seen, I think, in the light of much the same rational criteria as lend at least initial credence to the falsifiability criterion itself. Of course there are important differences between the ways one "tests" metascientific as opposed to scientific accounts—the former's refutability can less easily be justified in terms of providing needed input from the world of sense experience, since their "tribunal" typically comprises case value judgments rather than observations. But there seem to be solid rational grounds for desiring maximal criticizability and assessability in *any* constructive systematization or theory, and the remarks made by Popper, Feyerabend, and others concerning the eschewal of dogmatist tendencies bring this common rational norm to light. The attainment of truth in science, or of an adequate codification in metascience, cannot be hindered by processes that work to obscure the potential failings of systems constructed with such aims in mind. Indeed, this seems the very norm that underlies our favorable valuation of clarity and transparency of language and argument, and of the utility we attach to making presuppositions clear.

The structuring of Popper's program gives it the clear virtue of enhanced assessability. It attempts to indicate clearly how we are to make inferences regarding lower-order norms and decisions, and it tries to bring out its own most basic valuational presuppositions and their role in the resulting systematization. Popper's is a far more *aggressive* strategy. If successful, it would provide an elegant, systematic justificatory account of science. Even given its shortcomings—or, as many contend, its failures—it provides significant advantages over Kuhn's account: for these partial or total failures are more clearly detectable and the lessons learned from them more evident. That an intuitively plausible attempt to clarify our concept of science fails leaves us with a gain in understanding of that concept in the process: what seemed

workable did not work out, and hence something else must be tried. (Of course I am only assuming the worst for Popper's program, since its seeming advantages even in the face of failure seem to speak most strongly for the general strategy it embodies—a final, negative verdict on falsifiability is far from settled.)

I think it is fair to say that Popper sees in Kuhn's approach much the same overconcession to relativism that he decries in Duhem's conventionalist view of science (see, e.g., Popper, 1959, Ch. 4). That is, he fears (with Plato) that the matter of justification will simply be circumvented in the process. And I think he is correct: for Kuhn's program never seems to attempt, much less complete, any concrete *task* of justifying. Even with Hempel's supplementary remarks, Kuhn's position still seems to suffer substantially the same afflictions noted at the outset. We are directed to collect historical data regarding the criteria scientists actually use to guide their work, codifying these criteria in a limited way by attempting to formulate and order them to some degree. Indeed, this sort of limited ordering might seem to count as what Goodman regards as justification, except that Goodman's own work on the problem of confirmation builds on his own and others' serious and complex efforts to deepen our understanding of this aspect of science by seeking a relatively small set of fundamental principles on which a highly systematic codification is based. And it is not clear that codificaton without this sort of structuring can be regarded as justification at all: the "getting down to fundamentals" seems an important feature of our original notion of justification that is worth preserving in any reconstrual of it—and it is preserved in the albeit relativistic accounts of Goodman and Rawls.

Something seems evidently gained from Kuhn's inquiries, despite the absence of programmatic structure. The reason for this, I believe, is that Kuhn's work fits, albeit implicitly, into a guiding background program: the program, or set of programs, against which he is reacting—including that of Popper. We learn from Kuhn, I think, precisely insofar as his more careful look at the practice of science presents problems for the normative proposals of these earlier programs. As Hempel notes, this is a valuable antidote to philosophical tendencies to explicate the norms of science without attending closely to scientific practice: it is necessary to canvass the practice quite thoroughly if our reflective equilibrium account is to be ideally self-critical. But there seems to be a limit to the potential yield of this sort of negative critique, unless it is part of a continuing positive attempt to reivse and replace older programs in light of their failures.

Kuhn, I suspect, has too easily abandoned the hope of attaining a systematic justification of science that might promise to make substantial positive improvements in our understanding and appraisal of science. It may well turn out that a lot of what scientists do resists codification, or rests on

"tacit knowledge," and is beyond justification (cf. ftn. 4). However, we want to be sure that this is a genuine result, and not the mere consequence of having ceased to try.

In addition to valuing the assessability that structure brings to justification, I believe Popper also fears the loss of a necessary degree of *independence* of our critical perspective of science from science itself. In this, again, I agree with him. But the degree and nature of critical independence is unclear given the pragmatic rejection of essentialism he and Kuhn both expound. The achievement of genuine critique by such a "circular" account is a fundamental problem that any "bootstrap" approach to justification and rationality must confront. The circle of justification in which Kuhn moves seems too small, but just what is to be gained by Popper's attempts to widen the circle? The gain, I think comes to this: Popper's approach sets science in a wider context of intuitive rational and ethical valuations: he makes it clear that certain background assumptions, albeit themselves not fully systematized or clear, are being made, and that science is being made answerable to them in metascience. This appeal to the wider background of rationality is also evident in Hempel's criticisms of Kuhn's metascientific account—in his insistence that a causal connection must be shown between norms and rational deliberation if the norms are properly to be regarded as rational guideposts. Also, behind the Goodmanian notion of justification as codification we saw the rational valuation of a more systematic and efficient codified practice over its uncodified ancestors.

This general background of value judgments regarding rationality is important for assessing questions of justification, and I believe the value of thus widening the circle can be seen in the light of the reflective equilibrium account outlined above. These norms to which the metascientist appeals are not regarded as fixed, but instead as a general, evolving normative background which, despite its shifting character, is still our "ultimate arbiter" of justification. And just as the basic aims of science are clarified by attempted codifications, successful or unsuccessful, tested against this background, so our notions of rationality are improved by our attempts to pull ourselves up by a variety of bootstraps. The task of showing the rationality of scientific practice is inextricably interrelated with the task of learning what rationality is; in part, from science. The best one can ask for on this pragmatic view of justification is an increasingly more systematic understanding of the important relationships among elements of this background and the various areas of rational inquiry: science, theories of value and rational choice, and so on. Each individual area is better understood, and better rationally justified, by its increasingly more systematic interrelation with this background and with other attempted formalizations.

In this general context the need for structure seems just as important. Without some effort to systematize and test, there seems no evident way to

make much of a gain over what we start with: namely, a rough set of intuitions about rationality, proper scientific procedure, and so on, and a rough idea of how to order practice in accordance with them. Moreover, one wants to perceive as clearly as possible the relationships between our judgments of scientific rationality and other contexts in which parallel judgments about the rationality of other sorts of behavior are made: for some of these judgments broach ethical concerns that seem well outside the immediate context of evaluating science, and yet are such that the input of the lessons learned from science cannot be overlooked. These lessons play an important role in the attainment of a broader understanding of human rationality and in the clarification of norms that govern other important psychological, legal, and political contexts—but given this, the scientific input cannot be too uncritically accepted.

Notes

1. The present context is not affected, I believe, by whether one takes systems, theories, or hypotheses as the entities subject to falsification.

2. Popper and others also challenge Kuhn's account of normal science, arguing that it is more revolutionary than Kuhn claims. I shall not concern myself here with these and other albeit important matters of historical fact, as my purpose is instead to inquire as to how certain facts come to be regarded as salient in the first place. It seems to me that disputes about the adequacy of a historical analysis make sense only after the (perhaps conflicting) criteria of selection have been clarified.

3. Popper's earlier contention that evolutionary biology is unfalsifiable and hence metaphysical has been regarded by critics as a serious weakness for the falsifiability criterion construed even as stating only a necessary condition. However, Popper has recently changed his views, and now contends that evolutionary biology is falsifiable. See his "Natural Selection and the Emergence of Mind," *Dialectica* 32 (1978): 339–355.

4. Hempel (1981, p. 402) limits the applicability of Goodman's account only to cases in which "fairly precise rules can be stated that are acknowledged and observed in scientific practice." This would make justifiable, in this sense, only "fairly narrow and specific inductive problems, such as measuring a quantity, or testing a statistical hypothesis. . . ." However, I see no evident reason thus to limit ourselves. All that seems necessary are some at least roughly formulable principles that are offered to systematize an already existing judgmental practice (which may or may not already comprise stated rules on the basis of which presystematic judgments are made). If justification consists in a "fit" between principles and intuitions and in an improved practice, why should such justification presuppose *precise* formulation? However, Hempel's point can be granted, I think, without severely undermining hopes that the Goodmanian view can be applied to the broader contexts of justification considered in this paper. For if one demands, with Hempel, sufficient *explication* prior to justification, such explication proceeds in the very same reflective fashion on Goodman's account. Indeed, as Goodman points out: "The interplay . . . between rules and particular inductive inferences is simply an instance of this characteristic dual adjustment between definition and usage, whereby the usage informs the definition, which in turn guides extension of the usage." (1973, p. 66)

5. Rawls (1971), pp. 17–22; cf. Goodman (1973), pp. 62–67. The background of this view is most fully developed by Morton White in *Toward Reunion in Philosophy* (Cambridge: Harvard University Press, 1956), see especially pp. 254–268. In essence it is the extension to normative contexts of Quine-Duhemian epistemology—an epistemology, by the way, that Popper generally accepts, but whose relativistic implications he tries to combat in the manner indicated in this essay (see the concluding section). An excellent account of Goodman's views on justification may be found in Richard Rudner's article on Goodman in *The Encyclopedia of Philosophy*, vol. 3, ed. Paul Edwards (New York: Macmillan, 1967), pp. 370–371.

6. The intuitive judgments are "presystematic" in a logical rather than a temporal sense—as they continue to function in the assessment of a system after it has been initially developed.

7. Popper's primary objective is to explicate the notions of "testability" and "empirical content" in terms of falsifiability (1959, pp. 92, 113), that is, by determining the magnitude of these features in terms of the classes of "basic statements" that constitute "potential falsifiers" (I shall pass by the weighty technical difficulties involved here).

8. See especially "The Aim of Science" in Popper (1972).

9. Indeed, Popper's falsifiability criterion seems to mediate the background concept of justice with particular evaluative case judgments in much the same way that Rawls's principles of justice mediate our concept of justice with presystematic case judgments regarding the proper conduct of institutions and practices. In both cases there is the same generation of systematic first principles from background concept as well as the testing of the adequacy of the resulting systematizations by presystematic judgments in the light of which the principles are always regarded as subject to revision. And both programs draw similar benefits in virtue of this structuring. See Rawls (1971), especially pp. 11–22, 578–587; see also Rawls's "Justice as Fairness," *Philosophical Review* 65 (2) (April 1958) and "Outline for a Decision Procedure for Ethics," *Philosophical Review* 1 (1951).

10. The imposition of normative structure is not without its dangers. It is possible to be *too* bold in making conjectures before having adequately attended to the complexities of scientific practice. Popper may well be guilty of this, and we must regard with caution his attempt to lay down a supreme rule from the start (1959, p. 54). Among historically oriented philosophers of science there is concern that philosophical programs of this sort are fundamentally wrongheaded because they are initially launched from too fragmented an understanding of science, with the result that philosophical explications of, say, simplicity are in their view too belabored and artificial. Many doubt that any scientific utility can be served by current philosophical endeavors to codify or justify scientific practice.

Thus there appears to be a growing divergence—manifest in the debate between Popper and Kuhn—between two schools of thought in philosophy of science: one confronting the traditional problems of the logical empiricists, and the other abandoning much of the other's results and directing most of its energy to close historical study. By way of rapprochement, I should like to make the following two points.

(1) I do not see that the major advantages of an aggressive and self-critical learning strategy hinge on its boldness and grandness. If there is too much distance between the comprehensive programs of a Popper or Lakatos and actual scientific practice, or if it is the case that too little of scientific practice is understood by anybody to enable the derivation of a relatively small set of governing criteria pertinent to *all* scientific inquiry, there still seems to be an important place for at least limited conjectures

aimed at more modest justification and understanding of certain aspects of scientific practice. (See, e.g., Shapere's "The Character of Scientific Change," in *Scientific Discovery, Logic, and Rationality*, ed. T. Nickles (Dordrecht, Holland: D. Reidel, 1980), and "What Can the Theory of Knowledge Learn from the History of Knowledge?" *The Monist* 60 (1977): 488–507. His notion of progressive understanding of science through a "rational feedback mechanism" seems to conform to the general reflective equilibrium idea.)

(2) I do not believe the value of philosophical codifications of science rests fundamentally on their current or long-run scientific utility. For even where it is of little use to scientists to grasp or value such systematizations, there remains the other side of the reflective-equilibrium dynamic: that is, the improved understanding and accounting of the structure and aims of science that those with a philosophical interest in metascience can achieve. (This group could include the philosophically motivated scientist, though he might not thus be thinking qua scientist.) Given that I have chosen to exploit certain parallels in Rawls, this point can be missed: for although an improved reflective understanding of justice is one of Rawls's goals, the improved governance of practice, the clear understanding of his principles of justice by those they govern, are crucially important features in his contractarian account. The scientific codification, on the other hand, might have little use for the "governed"—but these formulations and systematizations (as well as those of rational choice) can serve other legitimate philosophical objectives.

11. I view the backgrounds of rational, ethical, and aesthetic valuation as inextricably interrelated. However, I shall not broach this issue here.

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The Conservatism of "Pseudoscience"

ROGER COOTER

For some time historians have recognized that the driving underground of hermeticism, alchemy and magic in the seventeenth century was integral to the process of rationalizing and legitimating the then newly-altered social order and its intellectual controls. Only recently, however, has it begun to be realized just how "socially imposed and self-consciously accepted" was the distinction between "correct" and "incorrect" knowledge made by the second generation in the Scientific Revolution, and how effectively that distinction served to obfuscate the social and ideological origins of modern science and its methodology.[1] As we are now beginning to understand, it was chiefly through the drawing of that distinction that the new way of knowing nature was able to establish itself as a universal and value-transcendent touchstone of truth, reason and rationality.

This paper, extending the discussion that has taken place on the role of pseudoscience in the seventeenth century, aims at formulating a general thesis on the role of pseudoscience in modern society. By "role" what is meant here is not the significance of a particular pseudoscience for those involved with it (although that will be partly the concern when we turn to the historical focus of this paper, the classic Victorian pseudoscience of phrenology). "Role" here refers, rather, to the function that the appellation "pseudoscience" serves for those who deploy the term. And the thesis I wish to put forward is simply this: that at least since the beginning of the consolidation and ossification of the capitalist order in the seventeenth century, the label "pseudoscience" (or the appropriate synonym) has played an ideologically conservative and morally prescriptive social role in the interests

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of that order. Further, I want to suggest that whenever and wherever "pseudoscience" is labelled, restraint is placed upon the creation of reality.

A convenient starting point is the definition of pseudoscience offered by Jeffrey Blum in *Pseudoscience and Mental Ability: The Origins and Fallacies of the IQ Controversy*. For the most part, this definition is characteristic of the way the term is deployed by today's liberal-minded. Pseudoscience, he says, is the "[p]rocess of false persuasion by scientific pretense." Its existence is dependent on the joint occurrence of (1) grossly inadequate attempts at verification and (2) the successful dissemination of the unwarranted conclusions drawn from those attempts at verification. "The label 'pseudoscience,' " he continues,

> becomes pertinent when the bias displayed by scientists reaches such extraordinary proportions that their relentless pursuit of verification leads them to commit major errors of reasoning. Simple tautological fallacies, a refusal to consider obvious alternative explanations, and the deliberate or unconscious falsification of data constitute some of the basic features of pseudoscience. [2]

Blum also tells us how pseudosciences work: usually they are associated

> with individual cranks working in isolation to promote their eccentric theories. Most instances of pseudoscience conform to this description and involve nothing more than the development of an idiosyncratic cult. When social circumstances are favorable, however, the perpetrators may acquire sufficient resources and respectability that they appear to the society generally as legitimate scientists rather than cranks. [3]

The obvious historical inadequacies of this use of "pseudoscience" scarcely need dwelling upon. As it is difficult to regard Newton as a crank because of his study of alchemy, so, as Barry Barnes has demonstrated, it is difficult to regard Arthur Jensen's scientific work on heredity as any less a product of a disinterested search after truth than that of Jensen's scientific opponents W. Bodmer and L. Cavalli-Sforza. [4] As for the isolation of the "crank," while it is hardly the case, at least before the second half of the nineteenth century, that anyone in scientific discovery worked in close professional association, [5] on the other hand it can no more be said of the "crank" than of the orthodox scientist that he or she operates in a social or cultural vacuum. Nor do I think that with this example of the use of the label "pseudoscience" we need to deliberate any further on its consistently perjorative nature. It is rather to the unstated ideological implications of the deployment of the label that I would like to turn.

Although recent debate on the issue of ideology and science has been stormy, complex and highly politicized, a number of gains have been made through it. One of these is the overcoming of much of the former difficulty of agreeing on what is meant by the use of the term "ideology." Historians of

science of many different political persuasions would now generally agree that by "ideology" we mean the partial view of nature and human nature expressed by a group or class which informs perception and conceptualization. It is not meant that ideology must be false nor that the reality of nature is necessarily distorted through ideology. The usage is Marxist-informed but it is non-perjorative and non-epiphenomenal, denoting simply the social, political, metaphysical, and theological or philosophical superstructure that must accompany every economic system.[6] According to this view, science as an intellectual formation and ideology as worldview can never be separate realities or autonomous "things" merely interacting, but must always be mutually constitutive of each other or interpenetrating to form a seamless web.

For many commentators on pseudoscience, however, idelogy retains the implication of distortion and falsification: "ideology" is the external social and political thing that lurks in the shadows and infiltrates objectivity; it is what can render science into pseudoscience and what is held to distinguish science from "scientism."[7] This narrow and trivial conception of ideology is precisely that which serves to deflect attention from the real ideological function of Blum's and others' exposés of pseudoscience, for in deploying this conception of ideology they conceal what is implicit in their exposés: the belief that there actually is out there somewhere a real, objective (non-ideological) body of truth called Science. By dogmatically asserting the existence of an imagined opposite—truncated "ideological" pseudoscience—one judges as one names and obscures the fact that modern science itself was and remains the product of men in the activity of historically specific social relations. As Lukác pointed out as long ago as 1923, modern science and its methodology were the chief instruments in reifying, and hence making to appear as objective, the social relations upon which our science was reared.[8]

When armed with this narrow view of ideology the student of pseudoscience is restricted from entering into any kind of relativist perspective on scientific truth, such as that provided by ethnomethodologists of science.[9] Completely foreign to Blum is the latter's Wittgensteinian interest in showing how scientific meaning and conceptualization are dependent upon the social context (and political interests) in which they are embedded. Denied access to the perception of scientific truths as cultural artifacts socially negotiated and organized in specific milieus, commentators such as Blum merely take "correct science" as the subject to explain "false science." Hence they commit themselves not to a sociology of knowledge, but to a sociology of error.

Of all so-called pseudosciences, none perhaps has committed so many to a sociology of error as phrenology, for none has been more frequently relied upon to elaborate the difference between true and false science. Yet no body

of scientific knowledge is perhaps better suited to lead one into a sociological understanding of knowledge than is phrenology, for when its history is considered non-teleologically and non-evaluatively, no body of scientific thought so readily permits the labelling "true" and "false" to be revealed as socially negotiated. For both these reasons phrenology lends itself to my cautionary tale on the deployment of the term "pseudoscience." For these reasons too, it is appropriate to broach the subject here by bouncing off from its actual history its received historiography.

Until very recently most historical accounts of phrenology were written within the positivist use/abuse framework.[10] The aim, intentionally or unintentionally, was not the writing of history, but the legitimating of the wisdom of value-free modern science. Consequently these accounts have given scientistic capital to dozens of other attacks on perceived scientifically-related bogies. Karl Dallenbach, for instance, in an article in the *American Journal of Psychology* in 1955 sought to deride the scientific pretentions of psychoanalysis by likening it to phrenology, and the same path has more recently been trod by the Nobel scientist turned literator P. B. Medawar.[11] In his essay on the IQ racket, the neurobiologist Steven Rose refers to Hans Eysenck as operating in "the manner of a nineteenth-century phrenologist" as if this comparison utterly vanquished Eysenck's credibility.[12] In fact it invalidates nothing, since Eysenck himself in his popular works on the "facts and fictions in psychology" justifies his scientificity by a similar contrast with phrenology.[13]

The positivist motives of both the left and the right in so deploying phrenology are patently obvious, and it has been partly because of this that revisionist historians of society and medicine have felt a strong need to rescue the subject.[14] These historians have performed a good service; they have shown (much in the manner of Darnton's splendid study of mesmerism in France)[15] that in its early-Victorian social context phrenology was very far from being the outlandish nonsense that Whig historiography implied through its teleological evaluation of the past from the present. By observing the interest in phrenology of countless illustrious contemporaries from A. R. Wallace, Robert Chambers and Samuel Smiles, to Comte, Spencer and Marx, they have shown that phrenology's impact and significance at both popular and intellectual levels was far deeper than had been previously assumed.[16]

Yet, paradoxical as it may seem, it cannot be simply inferred from this that phrenology was therefore scientifically legitimate in its context. Since the contextual evidence for phrenology being regarded in Britain as "a sick man's dream"[17] is in fact overwhelming, the Whig historians of phrenology were perfectly justified in calling phrenology pseudoscientific. From the moment of its first appearance in Britain, Gall's theory of brain and science of character were regarded as physiologically, craniologically and philo-

sophically visionary as well as socially, morally and theologically dangerous. Thomas Brown, in a well-known review of the theory in the *Edinburgh Review* in 1803, set a precedent for many far more passionate commentators with his conclusion that it was an unconvincing "species of physiognomy" unjustified by anatomy. Before 1820 phrenology was regarded with disdain in nearly every medical and literary journal while in popular prose and poetry, theatricals and cartoons it was satirically lambasted. And this was far from being just an initial Anglophile reaction to a Franco-Germanic theory. A sustained stream of irreproachable evidence rationally deducing phrenology's scientific failing flowed from Britain's most distinguished contemporary scientists and philosophers. Though nationalism was not absent from these critiques, it was not essential to them and was not in fact much relied upon. More typical was the dispassionate critique such as that presented by the physiologist Peter Mark Roget in his article on "Craniology" written in 1818 and published in the sixth edition (1824) of the *Encyclopedia Britannica.* In this, the case against phrenology was divested of emotional anti-materialism and anti-fatalism and was conducted on the basis of supposedly sound, neutral physiological evidence. Gall's basic physiological premises and his cerebral anatomy were even praised, while his theory of cerebral localization and the attendant craniology were torn to shreds.

By 1842, when Roget's article was republished unaltered in the seventh edition of the *Britannica* under the new title of "Phrenology," the scientific and scholarly backing for its opinions had been impressively expanded. In books, in articles in leading scientific and literary journals, dictionaries and encyclopedias, and in lectures delivered in Royal Societies, the doctrine was attacked in the 1820s and '30s by such eminent authorities as Dugald Stewart, Charles Bell, Sir Everard Home, Francis Jeffrey, Sir William Hamilton, James Cowles Pritchard, John Bostock and James Copland among many others. And, as if these attacks weren't enough, at the same time came the well-substantiated opposition to the doctrine by the French physiologists Francois Magendie and J.P.M. Flourens, the Berlin physiologist Karl Rudolphi, and the professor of physiology at Columbia University, Thomas Sewall. Whig historians, then, however misguided in their motives, were thus quite correct to call phrenology "pseudoscientific" in the sense of it being "discredited knowledge" in its own time. Their "only" mistake was to suppose that because of this phrenology must lack serious historical worth.

Just how wrong this supposition is becomes manifest as we swing round to what the revisionists assay, although it should be added that the revisionists neither understand nor appreciate the implications of the Whigs' error. Their primary intention after all is with the writing of accurate history, not with the lessons or the sociological insights to be derived from it. This aside, what they have amply proven is that while the highly-qualified opposition to phrenol-

ogy was being mounted, the phrenology movement was advancing by leaps and bounds. There is not space here to spell out the many facets of this movement, nor is it so necessary given the many recent publications. Suffice it to say that the institutionalization of phrenology was significant both in the narrow sense of formal organizations at national and local levels and in the sociological sense of establishing a knowledge/behavior base for standardized collective reference and action.[18]

For present purposes it is important to note only that the intellectual leadership of this movement was heavily weighted with medical men and that it was they who were most prominent in popularly disseminating phrenology among the self-styled "thinking classes" between the 1820s and 1940s. One finds, for example, that in the national Phrenological Association (which was formed belatedly in 1838), of the 218 members for whom we have occupations, the largest proportion—33%—were medical men. Local phrenological societies reflect a similar or greater preponderance of this professional group, and according to one phrenological statistician, at least a third of the one-hundred writers on phrenology in Britain that could be identified in 1838 were either physicians or surgeons.[19]

Given that phrenology was a physiological psychology, it is hardly surprising that much of the movement's early momentum should have come from the medical profession. Nor is it remarkable that by the late 1820s most leading medical spokesmen and educators were actively supporting the science along with the majority of medical journals (the *Lancet* chief among them).[20] After all, in undermining the Cartesian proscription on the scientific study of mind by uniting mind with neurology on the one hand and the biology of adaptation on the other, phrenology seemed to transform abstract metaphysical conceptions of mind into organic realities which were fully amenable to the understandings and practical interests of medical men. Moreover, phrenology's veracity seemed derived only through strict adherence to the empirical methodology of Bacon. This intellectualist explanation should not of course be taken as the full reason for the medical professions' support of phrenology; in fact this explanation is not really intellectualist at all, but compellingly social, since phrenology actually purported to maximize the competences of medical men to predict and control behavior. Here, however, it merely wants to be emphasized that in the context the reasons for supporting phrenology could be held to be entirely rational and scientific. Thus it is quite mistaken to transcend the historical context and brand the thought of antiphrenologists as "scientific" and that of the phrenologists as "scientistic." As suggested by the fact that it was Britain's most distinguished scientists and philosophers who conducted and mightily sustained the attack on phrenology as pseudoscience, antiphrenologists no less than phrenologists might be seen as having been anxious to defend

interests above and beyond what normally would be called scientific.

What these interests were becomes clear when we socially map the participants.[21] In brief (and comparing here only the scientific-medical opponents and supporters of phrenology), what stands out in bold relief are the differences in age, economic and social security, religious affiliation and politicization. The supporters of phrenology were on the average twenty years younger than their opponents. They were socially marginal and economically insecure: few had upper middle class or aristocratic backgrounds, had had elitist educations or belonged to elitist institutions, as did many of the antiphrenologists; and while some managed to struggle into university positions during the period in which they advocated phrenology, these were few in number and were generally confined to the less prestigious postings. In terms of religion: as opposed to the antiphrenologists' High Church involvement and firm attachment to socially undisturbing Paleyian natural theology, virtually all of the supporters of phrenology were either avowed materialists, proponents of "the grander view" of the Creator (sometimes bordering on token deism), or were vocal dissenters. These positions on religion were at the same time polticial postures for which party labels serve as insufficient guides. For the most part the supporters of phrenology can be seen as Radical partizans, while the antiphrenologists can be seen as having a more conservative attachment to Whig reformism.

The fuzziness of this last distinction reminds us, however, that the social and ideological conflict between phrenologists and antiphrenologists cannot be reduced simply to one of class nor exclusive cosmologies and worldviews. To refer to exclusive cosmologies might imply that the phrenologists were providing the intellectual rationales for social structures and relations totally different from those of the antiphrenologists. But this was not quite the case, even though between the two groups elements of a genuine conflict between aristocratic and bourgeois interests can on occasion be isolated. The antiphrenologists for the most part shared with the phrenologists an optimistic uniformitarian faith in gradual progress, in social homogeneity, stability, and hierarchical order, and shared with the phrenologists a non-literalism in Biblical matters and a view of a lawful rational universe presided over by a benign Deity.[22] Thus, although at first sight the reaction to phrenology by the orthodox scientific-philosophical community (especially in Edinburgh) bears a striking resemblance to the reception given to Velikovsky's ideas by our own scientific community,[23] in actuality the outlook of phrenologists and antiphrenologists was very far from being parallel to that of today's catastrophists and uniformitarians. Though the conflict involved metaphysical and metaphorical sound and fury, it is an over-simplification to say that worldviews were in collision. The history of the conflict is far more subtle, involving in part the rhetorical build-up of caricatures which were then used by marginal men offensively and defensively for the rationalizations of the

industrial secular form of bourgeois capitalism. These caricatures distorted and polarized the views of phrenologists and antiphrenologists, making the phrenologists appear as progressives for an open society allied to the mobile fortunes of commerce, and the antiphrenologists appear as the retainers of a static and privileged aristocratic society allied to inherited agrarian wealth. What in most cases were differences between different factions of the same class over *degrees* of social re-ordering and the pace of social change were, through the dialectical spiral of the phrenology debates in the context of the further emergence of urban industrial society, construed as competing class views over *kinds* of social orders. The point is historically important but is not of central concern to us here, for caricature or not, the consequence of entering into debate on phrenology was the same: the impossibility of any participant remaining politically, philosophically, or theologically detached. No matter whether the advocacy or denunciation was polemical or factually dispassionate, that advocacy could never be intellectually autonomous or be *just* scientific and medical.[24]

Once we disengage phrenology from the clutches of the use/abuse model and perceive it in this way as a component in a social and ideological strategy, we can identify the conservatism in the contemporary labelling of it as "pseudo." The labelling can be called "conservative" not because at a political level the phrenologists happened to be more radical, but more fundamentally, because those in or identifying with the scientific and philosophical elite who deployed the label were seeking to discredit what they saw to varying degrees as socially threatening and, by this process, to legitimate and defend certain human and social interests and the neutrality of the knowledge that served them. Would space permit, the thesis could be amplified by revealing how phrenologists themselves attempted to separate a "pure" version of their science from a purported "corrupted" ("pseudoscientific") version once *their* sociopolitical interests had become institutionalized and required protection from below. Even without this elaboration, however, it should be clear that it is the basic defense of interests, rather than the specific political color of those interests that warrants the use of the term "conservative."

To sum up: I've been arguing that the deployment of the label "pseudoscience" always acts to conserve because, almost by definition it seems, the label appertains to perceived forms of sociopolitical deviance or to what at any moment in history may be deemed morally illegitimate.

More specifically, I have argued that the deployment of the label "pseudoscience" acts conservatively because, unwittingly, it legitimates the partial view of reality that is mystified and mediated through science. To identify and attack "pseudoscience" is both to support the reproduction of the capitalist structures and relations that emerged constitutively with modern science and to reproduce the social process of concealment originally

conducted in the seventeenth century; isolation and exploitation of "pseudoscience." Through the deployment of "pseudoscience" the partial view of reality asserted by modern science remains thoroughly mystified under the socially-constructed metaphysic of "truth" and "certainty."

Because all post-seventeenth century attacks on "pseudoscience" must similarly conserve and protect the ideology embedded in science, it does not matter whether the deployment comes from the left or from the right or whether the social movements which frequently toss up the stipulation "pseudoscience" have explicitly or implicitly to conserve a great deal or very little. Although it has been necessary here to compress and abridge, the sociopolitical vicissitudes of phrenology's history illustrate how new demands upon the bourgeois order always call forth new deployments of "pseudoscience" which work overtly and covertly in the interest of conserving that order.

Since all knowledge of external nature is made by men and is socially constructed, the identification and criticism of any particular body of knowledge as "pseudoscientific" must count as a defense of some other body of knowledge. In the case of phrenology, as in the contemporary situation with IQ testing, claims and counter-claims of "pseudoscientificity" (i.e. claims and counter-claims of the social infiltration of the other's knowledge) constitute the defense of human and social interests in the name of an "objective world" (however politically or otherwise defined). To label "pseudoscience" in the interest of creating this "objective world" must be seen as deeply conservative, for it is a constraint on man's creative activity—a corset on any alternative view of man making, not only his own history, but also his own reality.

Since bodies of scientific knowledge are thus criticized by identifying the presence of social interests in them, and since that identification is essential to stipulating that certain bodies of knowledge are not knowledge at all, but are "ideology," "pseudoscience," "error," etc.,[25] it follows that the study of "pseudoscience" has value not for its own sake nor for the sake of legitimating as "objective" what is actually political in modern science, but for the sake of revealing more about the science and society that negotiationally *defines* pseudoscience through its interactions with it—both past and present. It is only through this kind of understanding that we can hope to remove the iniquities that are falsely and misleadingly attributed to "pseudoscience." Speaking practically, insofar as the historical aim is description rather than prescription it would be preferable to have the term "pseudoscience" replaced in our vocabularies with something like "unorthodox science" or "non-establishment science," for this would more correctly imply relative differences in scientific social perception rather than categorical methodological opposites or matters of truth and falsehood. If we must use the label "pseudoscience" than let us at least acknowledge that that usage, whether

from the left or from the right, is historiographically as ideological and covertly mystifying as its usage by any figure in history. For what does the focus on "bad" science accomplish, but the obfuscation of the ideological power of "good" science? The task before the social historian of "pseudoscience," surely, is not the reproduction of this essentially political process but its avoidance and exposure.

Notes

1. Everett Mendelsohn, "The Social Construction of Scientific Knowledge," in Mendelsohn, et al., eds., *The Social Production of Scientific Knowledge* (Dordrecht, Holland and Boston: D. Reidel, 1977), 3-26, esp. 17-20; see also David Dickson, "Science and Political Hegemony in the 17th Century," *Radical Science Journal*, 8 (1979), 7-37.

2. Jeffrey Blum, *Pseudoscience and Mental Ability: the origins and fallacies of the IQ controversy* (New York: Monthly Review Press, 1978), 12-13.

3. Ibid., 19.

4. Barnes, *Scientific Knowledge and Sociological Theory* (London: Rutledge, 1974), 134.

5. See Susan Cannon, "Professionalization," in her *Science in Culture: the early Victorian period* (New York: Dawson, 1978), 137-165.

To anticipate the critique that follows, note that Blum's defense of "real science," portrayed as collective enterprise, is a recapitulation of Bacon's ideal of scientific organization and is therefore, unbeknownst to Blum, a defense of capitalist practices and forms over alternative practices and forms. On the formal similarity between Bacon's scientific method and the capitalist labor process see Dickson op. cit. (note 1), 11-12.

6. Such a usage, derived from Althusser, may be found in Mary Hesse, "Criteria of Truth in Science and Theology," *Religious Studies*, 11 (1975), 396. My own usage is derived more from Henri Lefebvre, "Ideology and the Sociology of Knowledge," in his *The Sociology of Marx*, trans. Norbert Guterman (Harmondsworth: Penguin, 1966), 59-88 and reprinted in Janet L. Dolgin, D.S. Kemnitzer and D.M. Schneider, eds., *Symbolic Anthropology* (New York: Columbia University Press, 1977), 254-269; Clifford Geertz, "Ideology as a Cultural System," in David E. Apter, ed., *Ideology and Discontent* (Glencoe: Free Press, 1964), 47-76; and the writings of Bob Young, of which see: "Science Is Social Relations," *Radical Science Journal* 5 (1977): 65-129, which contains a useful bibliography of sources on science and ideology, including Young's several other works. See also Jorge Larrain, "Ideology and Science," in his *The Concept of Ideology* (London: Hutchinson, 1979), 172-211.

7. Throughout this paper "scientism" refers to the transformation of positivism (the natural philosophy that combines the traditions of empiricist and rationalist thought) into social philosophy on the basis of which society is explained and interpreted. Scientisms are extrapolations from well-established scientific domains to social domains, but since science itself is constructed under social conditions and subsumes these conditions, scientisms may be said only to accomplish overtly for social legitimation what sciences do covertly. See: Luke Hodgkin, "A Note on 'Scientism,' " *Radical Science Journal*, 5 (1977), 8; and David Dickson, "Technology and Social Reality," *Dialectical Anthropology*, 1 (1975), 34-37. Cf. the orthodox distinction between science and scientism in F.V. Hayek, "Scientism and the Study of Society," in his *The Counter-Revolution of Science: studies on the abuse of reason*

(Glencoe: Free Press, 1955), 129–142; George Eastman, "Scientism in Science Education," *The Science Teacher*, 36 (1969), 19–22; and Robert B. Fisher, "Science and/or Scientism," *Science, Man and Society* (Philadelphia: W.B. Saunders, 1971), 43–44.

8. Georg Lukacs, *History and Class Consciousness* (1923; new ed. London: Merline, 1971), 7–11, 89–98 et passim. The relevant passages are cited in Gareth Stedman Jones, "The Marxism of the Early Lukács," *New Left Review*, 70 (1971), reprinted in Gareth Stedman Jones, ed., *Western Marxism: a critical reader* (London: New Left Books, 1977), 13–14. See also Robert M. Young, "The Historiographic and Ideological Contexts of the Nineteenth-Century Debate on Man's Place in Nature," in Young and Mikulas Teich, eds., *Changing Perspectives in the History of Science* (London: Heinemann, 1973), 398–399, 402, 405, 414, 430–434. (Note: reification should properly be understood as implying the process whereby the history of a thing is separated from its human origin and quality, i.e., as "misplaced concreteness" or the assuming of the objective entity existence of that which is actually "metaphysical.")

Lest there be any question, I am not saying that science is nothing but ideology, only that science is ideology as well as whatever else it is. If it is universally true that $E=mc^2$, it will remain so whatever the dominant ideology in any society, though it will have different meanings and uses.

9. See: David Bloor, "Wittgenstein and Mannheim on the Sociology of Mathematics," *Studies in the History and Philosophy of Science*, 4 (1973), 173–191; H.M. Collins and Graham Cox, "Recovering Relativity: did prophecy fail?" *Social Studies of Science* 6 (1976), 423–444; H.M. Collins, "The TEA Set: tacit knowledge and scientific networks," *Science Studies*, 4 (1974), 165–186; idem, "The Seven Sexes: a study in the sociology of phenomenon, or the replication of experiments in physics," *Sociology*, 9 (1975), 205–224. Particularly relevant applications of the "relativist" approach to the study of "pseudoscience" (though unfortunately unavailable to me during this paper's preparation) are H.M. Collins and T.J. Pinch, "The Construction of the Paranormal," and R.G. Dolby, "Reflections on Deviant Science," both in Roy Wallis, ed., *On the Margins of Science: the social construction of rejected knowledge*, Sociological Review Monographs (University of Keele: 1979).

I have been mostly informed in this paper by the historical applications of ethnomethodology in the studies by Barry Barnes and Steven Shapin. They have relied heavily on the insights of anthropology (especially that of Mary Douglas) to underline the fact that the question of truth and falsehood in science is neither here nor there; "truth," they argue, is relative to the social context of its use, and to question it historically is to avoid the more important problem of how scientific truth is constructed and how it functions in society. "Truth," as Mary Douglas reminds us in the words of Sir James Fraser, "is only the hypothesis which is found to work best." *Purity and Danger: an analysis of concepts of pollution and taboo* (London: Routledge, 1966), 24, quoted in Shapin, "Homo Phrenologicus: anthropological perspectives on an historical problem," in Barnes and Shapin, eds., *Natural Order: historical studies of scientific culture* (London/Beverly Hills: Sage, 1979), 44. For general criticism on the relativist position (on the grounds that not all truths are context-dependent because some a la Pareto's "residues" are universal and fundamental) see Steven Lukes, "On the Social Determination of Truth," and "Relativism: cognitive and moral," both in his *Essays in Social Theory* (London: Macmillan, 1977); idem, "Some Problems about Rationality," in Bryan R. Wilson, ed., *Rationality* (Oxford: Blackwell, 1970), 3–44, esp. at 39ff.

My own position is that what is argued as scientific "truth" or "certainty" is context-dependent (is invented, not discovered), but there is nevertheless a real world of non-ideological facts (external nature) outside one's consciousness (as Feuerbach argued against Hegelian Idealism). I agree with Marx, that "The human significance of nature only exists for social man" and therefore only through social change is the human significance of nature changed. (For relevant sources on this philosophical problem see Melvin Rader, *Marx's Interpretation of History* [New York: Oxford University Press, 1979], 108–109 and Alfred Sohn-Rethel, "Science as Alienated Consciousness," *Radical Science Journal*, 2/3 [1975], 80n. See also Raymond Firth, "The Sceptical Anthropologist?" in Maurice Bloch, ed., *Marxist Analysis and Social Anthropology* [London: Malaby Press, 1975], 29–60 [esp. at 31]; and Karl Figlio, "Chlorosis and Chronic Disease in Nineteenth-Century Britain: the social constitution of somatic illness in a capitalist society," *Social History*, 3 [1978], 168.)

10. For a review of the recent literature on phrenology and a fairly complete bibliography of past writings on the subject see my "Phrenology: the provocation of progress," *History of Science*, 14 (1976), 211–234.

11. Karl M. Dallenbach, "Phrenology versus Psychoanalysis," *American Journal of Psychology*, 68 (1955), 511–525; P.B. Medawar, "Further Comments on Psychoanalysis," in his *The Hope of Progress* (London: Methuen, 1972), 57–68.

12. Steven Rose, "Scientific Racism and Ideology: the IQ racket from Galton to Jensen," in Hilary and Steven Rose, eds., *The Political Economy of Science: ideology of/in the natural sciences* (London: Macmillan, 1976), 112–141, esp. 117–124.

13. Hans Jurgen Eysenck, *Fact and Fiction in Psychology* (Harmondsworth: Penguin, 1965) 130–131 (where the essay by Dallenbach cited above comes in for praise); *Sense and Nonsense in Psychology* (Harmondsworth: Penguin, 1958), 61 (which in the same breath links in mesmerism); and *Uses and Abuses of Psychology* (Harmondsworth: Penguin, 1954), 28–29.

14. See, in particular, David de Giustino, *Conquest of Mind: phrenology and Victorian social thought* (London: Croom Helm, 1975); John D. Davies, *Phrenology, Fad and Science: a 19th century American crusade* (New Haven: Yale University Press, 1955); Alastair Cameron Grant, "George Combe and His Circle: with particular reference to his relations with the United States of America" (Ph.D., Edinburgh, 1960); T.M. Parssinen, "Popular Science and Society: the phrenology movement in early-Victorian Britain," *Journal of Social History*, 7 (1974), 1–20; Angus McLaren, "Phrenology: medium and message," *Journal of Modern History*, 46 (1974), 86–97; Owsei Temkin, "Gall and the Phrenological Movement," *Bulletin of the History of Medicine*, 21 (1947), 2–15; and, from a different and deeper social perspective, the papers by Steven Shapin cited below (note 21).

15. Robert Darnton, *Mesmerism and the End of the Enlightenment in France* (New York: Schocken, 1970).

16. None of this is any longer in doubt and is strongly reinforced by the evidence given in my "The Cultural Meaning of Popular Science: phrenology and the organization of consent in nineteenth century Britain" (Ph.D., Cambridge, 1978). Because this study is forthcoming by Cambridge University Press, and because the point of this paper is not to marshal evidence but to argue on the basis of that evidence, I have in what follows avoided where possible detailed citations. The evidence upon which the rest of this paper is based is taken from the first two chapters of my study.

17. The dismissive phase of Bentham's: *The Works of Jeremy Bentham*, John Bowring, ed. (1843), vol. 7, 433–434.

18. The literature for the sociological sense of "institutionalization" is cited in Steven Shapin, "Social Uses of Science, 1660–1800," in G.S. Rousseau and Roy Porter, eds., *The Ferment of Knowledge: Studies in the Historiography of Eighteenth-Century Science* (Cambridge: Cambridge University Press, 1980), 93–139.

19. [Hewett Cotrell Watson], *Phrenological Journal*, 11 (1838), 263.

20. See my "Phrenology and British Alienists, c. 1825–1845," *Medical History*, 20 (1976), 1-21, 135-151.

21. Such a map, based on 28 major medical supporters of phrenology in Britain and 14 major scientific opponents, is discussed in Cooter, "Cultural Meaning of Popular Science," op. cit. (note 16), Ch. 1, pt. iv, with biographical details in Appendix A.

That the phrenology debates were about social and ideological interests was first made clear in Steven Shapin, "Phrenological Knowledge and the Social Structure of Early Nineteenth-Century Edinburgh," *Annals of Science*, 32 (1975), 219–243. Since then Shapin has moved beyond the prosopographical and rhetorical terrain of the phrenology debates and into their scientific substance to further illustrate the social and ideological strategies involved; see Shapin, "The Politics of Observation: cerebral anatomy and social interests in Edinburgh phrenology disputes," in Wallis, op. cit. (note 9), pp. 139–178. In "Homo Phrenologicus," op. cit. (note 9), with a heavy reliance on anthropological writings, Shapin has sought to work out "a social epistemology appropriate for the history of science" by ascertaining through the historical case of phrenology in the context of its use in Edinburgh the nature of the links between natural knowledge and social context; while in his " 'Merchants in Philosophy': the politics of an Edinburgh plan for the diffusion of science, 1832–1836," in J.B. Morrell and Ian Inkster, eds., *Metropolis and Province: British science 1780–1850* (London: Hutchinson, forthcoming), he endeavors to illuminate the political and logistic problems involved in the active diffusion of scientific ideas by a careful local study of the involvements of Edinburgh phrenologists in the Edinburgh Philosophical Association.

22. It is difficult to confine Roget (1779–1869), for example, to an aristocratic orbit. Though his uncle was Sir Samuel Romilly, and though he became a Fellow of the Royal Society in 1815 and was the secretary and editor of the Society's proceedings between 1827 and 1848, he was by association and marriage more closely allied to bourgeois merchantile sources of wealth and utilitarian philosophy. He is more representative of the members of the Manchester Literary and Philosophical Society or the Royal Institution, or the Society for the Diffusion of Useful Knowledge, of which he was, respectively, a member, a Governor, and a committee member. He was intimate with Bentham and other Philosophical Radicals; was Physician to the Manchester Infirmary in 1805 and later to the Northern Dispensary in London; and helped found the Manchester Medical School and the University at London. See D.L. Emblem, *Peter Mark Roget: the word and the man* (New York: Crowell, 1970).

23. See Robert Mcaulay, "Velikovsky and the Infrastructure of Science: the metaphysics of a close enounter," *Theory and Society*, 6 (1978), 313-342. Useful parallels can also be drawn from the literature on the early nineteenth-century debate in geology between catastrophists and uniformitarians; from that on the early twentieth-century disputes between the "biometricians" and "Mendelians" and between the Darwinians and Lamarckians; and from the literature on the current debate between hereditarians and environmentalists over IQ. See, respectively, Walter F. Cannon, "The Uniformitarian-Catastrophist Debate," *Isis*, 51 (1960), 38–55; Donald MacKenzie and Barry Barnes, "Scientific Judgement: the Biometrymendelism controversy," in Barnes and Shapin, op. cit. (note 9), 191-210; Arthur Koestler, *The Case*

of the Midwife Toad (London: Hutchinson, 1971); Jonathan Harwood, "The Race-Intelligence Controversy: a sociological approach," *Social Studies of Science*, 6 (1976), 369-394 and 7 (1977), 1-20; and idem, "Heredity, Environment, and the Legitimation of Social Police," in Barnes and Shapin, op. cit. (note 9), 231-251.

24. Cf. G.N. Cantor, "The Edinburgh Phrenology Debate: 1803-1828," *Annals of Science*, 32 (1975), 195-218 and idem, "A Critique of Shapin's Social Interpretation of the Edinburgh Phrenology Debate," ibid., 32 (1975), 245-256.

25. Shapin, "The Politics of Observation," op. cit. (note 21), p. 140.

Suggested Readings

As noted in the general introduction, this is not intended as anything like a complete bibliography. The works mentioned are generally standard sources, for the most part easily available, which may be of help at the next stage of the reader's research.

Standard sources on demarcation and related issues:

Duhem, Pierre. *The Aim and Structure of Physical Theory*. Translated by P. Wiener. Princeton: Princeton University Press, 1963.

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Kuhn, Thomas S. *The Structure of Scientific Revolutions*. 2nd ed. Chicago: University of Chicago Press, 1970.

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Lakatos, Imre, and Musgrave, Alan, eds. *Criticism and the Growth of Knowledge*. New York: Cambridge University Press, 1970. An especially good anthology, including contributions by Popper, Kuhn, and Feyerabend.

Popper, Karl R. *Conjectures and Refutations: The Growth of Scientific Knowledge*. London: Routledge & Kegan Paul, 1962.

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———. *Objective Knowledge: An Evolutionary Approach*. Oxford: Clarendon Press, 1972.

Recent articles of note:

Churchland, Paul M. "Karl Popper's Philosophy of Science." *Canadian Journal of Philosophy* 5 (1975): 145–156.

Doppelt, Gerald. "Kuhn's Epistemological Relativism: An Interpretation and Defense." *Inquiry* 21 (1978): 33–86.

Hempel, Carl G. "Turns in the Evolution of the Problem of Induction." *Synthese* 46 (1981): 389–404.

Naess, Arne. "Why Not Science for Anarchists Too: A Reply to Feyerabend." *Inquiry* 18 (1975): 183–194.

On "reflective equilibrium" see:

Daniels, Norman. "Wide Reflective Equilibrium and Theory Acceptance in Ethics." *Journal of Philosophy* 76 (1979): 256–282.

Goodman, Nelson. *Fact, Fiction, and Forecast.* 3rd ed. Indianapolis, Ind.: Bobbs-Merrill, 1973.

Rawls, John. *A Theory of Justice.* Cambridge: Harvard University Press, 1971. Especially Ch. 1, section 9, pp. 46–53, and Ch. 9, section 87, pp. 577–587.

On relativism and the sociological approach:

Barnes, Barry. *Scientific Knowledge and Sociological Theory.* London: Rutledge, 1974.

Mendelsohn, Everett et al. *The Social Production of Scientific Knowledge.* Dordrecht, Holland: D. Reidel, 1977.

Meiland, Jack, and Krausz, Michael, eds. *Relativism: Cognitive and Moral.* Notre Dame: University of Notre Dame, 1982.

Tiryakian, Edward A., ed. *On the Margin of the Visible: Sociology, the Esoteric, and the Occult.* New York: Wiley, 1974.

Wilson, Bryan R. *Rationality.* Evanston, Ill.: Harper & Row, 1970.

Young, Robert M., and Teich, Mikulas, eds. *Changing Perspectives in the History of Science.* London: Heinemann, 1973.

SECTION III

PARAPSYCHOLOGY

Introduction

Parapsychology seems to stand out as in some way unique among those topics that are commonly dismissed as "pseudoscientific"; the case of parapsychology seems importantly different. Many who scoff at Atlantis, Bigfoot, or the Bermuda Triangle nevertheless take parapsychological research regarding clairvoyance, precognition, telepathy, and psychokinesis quite seriously, though the reverse is rarely the case. The great bulk of the literature on other topics labeled "pseudoscience" appears in the form of glossy paperbacks designed for quick and perhaps uncritical reading. But parapsychology is represented by numerous tomes of research materials as apparently dry and dispassionate as any in astronomy or sociology. Other topics have their passionate and sometimes eloquent defenders. But parapsychology can boast scores of dedicated researchers who have devoted years and decades to diligent study and investigation.

Of course none of these characteristics will settle the status of parapsychology as science or pseudoscience. But they do serve to distinguish parapsychology at least superficially from other pursuits dismissed as pseudoscience, and thus may indicate that parapsychology presents a particularly troublesome case for attempts at demarcation.

Some, of course, will claim that these differences are to be expected; that parapsychology differs from other pursuits that have been labeled pseudoscience simply because it has *wrongly* been labeled pseudoscience. Although perhaps a fledgling science, or a science in which a great deal of both empirical and theoretical work remains to be done, its defenders will claim, parapsychology is nevertheless a *science*.

In the opening piece of this section, RUTH REINSEL presents the case for parapsychology. She offers a historical review of important results obtained concerning clairvoyance, precognition, telepathy, and psychokinesis, and discusses skeptical attacks and methodological modifications that have been

made in response to criticism. In the final section of her paper, Reinsel outlines contemporary trends in parapsychological research and makes some theoretical suggestions of her own.

One of the simplest charges leveled against any putative case of precognition, clairvoyance, or the like is that what is really at issue is merely a case of coincidence. But what does it mean to call something a coincidence? And what would justify or defeat a charge of coincidence, in general or in the case of parapsychology in particular? These are the issues GALEN K. PLETCHER attempts to address in the second selection. To attribute something to coincidence, Pletcher argues, is not to explain it or even to attempt to explain it; it is instead to maintain that no special explanation is required. His charge against parapsychology is that it has not yet shown that its "data" stand in need of special explanation, and here Pletcher faults attempts at explanatory theory as well as case studies and laboratory work in parapsychology. Until parapsychology can offer more consistent laboratory results, he maintains, or until it can present a more satisfactory explanatory framework, it will not have adequately responded to the simple charge of coincidence. A careful evaluation of Pletcher's conclusion may call for a review of the laboratory results surveyed in Reinsel's opening piece.

In the next two sections, ANTONY FLEW and JANE DURAN raise a number of important philosophical difficulties regarding parapsychology and parapsychological phenomena. ANTONY FLEW discusses in some detail three respects in which parapsychology differs "from all the established high-status sciences." Its domain, first of all, is only negatively defined: "psi-gamma," for example, "is at this time defined as precisely not the product of any means we can think of." Second, there is no repeatable demonstration that parapsychology has its own peculiar and genuine data to investigate: "The black record of fraud would not carry nearly so much weight against what might seem to be strong cases of psi; if only we possessed some repeatable demonstration of the reality of such phenomena." Third, Flew claims, "there is no even half-way plausible theory with which to account for the materials [parapsychology] is supposed to have to explain." Flew's is a wide-ranging discussion, as intriguing in its subsidiary treatment of disembodied minds and Hume's position on miracles as in its main line of argument. At no point does Flew simply label parapsychology a pseudoscience. But he does propose that if affiliation with the American Association for the Advancement of Science is to be thought of "as a recognition of actual achievement rather than of good intentions, then the Parapsychological Association is not yet qualified for admission, and ought now to be politely disaffiliated."

JANE DURAN pursues further a number of philosophical reasons that have been given for holding that precognition, clairvoyance, and telepathy are not possible at all. Using the work of C. D. Broad, she considers a set of

"basic limiting principles"—part of our everyday intellectual framework—that seem at least at first sight to be inconsistent with the claims of parapsychology. On careful consideration, however, she rejects a number of the charges; some of the philosophical arguments that have been leveled against parapsychological phenomena prove inadequate. But Duran holds that one particular charge, evident in Broad's first "limiting principle" regarding causality, is a telling one. "As long, at least, as our ordinary notions of causality remain intact, there seem to be strong philosophical reasons for holding that telepathy, clairvoyance, and precognition are not possible." In a final section Duran argues that even if these were accepted as possible, they could not legitimately be construed as means of knowledge.

At this point in the readings it will be clear that one standard philosophical objection to putative parapsychological phenomena—perhaps the strongest philosophical objection—is that such phenomena would violate basic principles regarding causality. Precognition, for example, would seem to demand "backward causation"; an effect (such as the vision of an airplane crash) would have to precede its cause (presumably the crash itself). But the notion of "backward causation," both Flew and Duran insist, is simply incoherent. Duran claims that part of what we *mean* by "*A* causes *B*" is that *A* precedes *B* temporally, and Flew maintains that "it must be irredeemably self-contradictory to suggest that the (later) fulfillments might cause the (earlier) anticipations."

It is this objection in particular that BOB BRIER and MAITHILI SCHMIDT-RAGHAVAN attempt to answer in the piece included here. With an eye both to a parapsychological experiment of their own and to proposals in physics regarding particles that travel backward in time, and drawing on the work of John Stuart Mill, Brier and Schmidt-Raghavan argue that a scientific view of causality need impose no temporal requirements of the sort urged by Flew and Duran. The claim that "backward causation" is logically impossible, Brier and Schmidt-Raghavan maintain, is based on confusions between asymmetry and atemporality and between changing the past and affecting the past. But they do speak of a "revision of the concept of cause," and in that regard the reader is advised to consider again Jane Duran's comments in the preceding piece concerning changes in the meaning of "cause."

The last recourse of the critic, it has been said, is to charge fraud. But unfortunately—within parapsychology as within other fields—this charge has at least on occasion been justified. Some cases of fraud have been mentioned in Reinsel's opening review and in Flew's work. In the final piece of this section, J. B. RHINE outlines and discusses the falsifications of Walter J. Levy, Jr., discovered in 1974. But Rhine also suggests categories and presents examples of "fraud-proof evidence of psi," and maintains that "these fraud-proof types of evidence, if now given precedence for a showdown on

the establishment of psi, will fill the need for the scientific mind that considers it." Rhine closes with some general proposals regarding the future of parapsychological research.

The readings of this section pose a number of issues in the philosophy of science that, though perhaps related to the problem of demarcation, also deserve careful consideration in their own right. What is demanded of an explanation, and is parapsychology able to supply it? What are natural laws, or scientific laws, and what is their role in explanation? What is the status of theoretical entities such as "psi"? What role do basic intuitive principles properly play in science? What is meant by saying that one event causes another, and does that allow a logical possibility of "backward causality"? Issues regarding causality and correlation, first broached in our initial examination of astrology, arise here as well.

It is also clearly of value to address again the issue of demarcation with parapsychology in particular in mind. *Is* parapsychology a pseudoscience? We began by saying that parapsychology seems to stand out as in some way unique among those topics that are commonly dismissed as "pseudoscientific." But precisely how does it differ?

Parapsychology: An Empirical Science

RUTH REINSEL

Although the historical roots of scientific parapsychology are tangled with nineteenth-century spiritualism, the history of parapsychology is to a large extent the story of its struggle to disassociate itself from the occult. The occult and parapsychology can be distinguished both in the choice of methods and in the choice of subject matter. The occult relies on traditional bodies of arcane lore interpreted with a healthy dose of personal intuition. Because of this combination of tradition and intuition, what are sometimes called the occult "sciences" should more properly be called the occult "arts." Parapsychology, on the other hand, relies heavily on objective evidence, quantitative evaluation, and the experimental method. The occult includes topics of popular appeal but of questionable validity, such as astrology, numerology, and magic. Parapsychology, on the other hand, restricts itself to the study of ways of gaining knowledge or affecting the world around us that do not involve the five normal senses. Since no known process or energy can account for the accumulating mass of parapsychological data on ESP, precognition, and psychokinesis, [1] these abilities are termed "paranormal." This, it is to be hoped, will be only a temporary designation, since it is possible that some explanatory principle remains to be discovered that will satisfactorily reconcile these phenomena with conventional science.

If dated from the founding of the Society for Psychical Research in London in 1882, parapsychology will soon celebrate its first century of development. Philosophers have been intimately connected with the development of parapsychology from its inception—witness the membership rolls of the English and American Societies for Psychical Research, which include William James, Henry Sidgwick, C. J. Ducasse, C. D. Broad, and H. H. Price—and it seems appropriate now for parapsychologists and philosophers together to evaluate the progress that has been made in this area. In

what follows I shall present a brief historical review, a survey of methodological difficulties and skeptical objections overcome, and a discussion of current techniques in parapsychological research. No honest appraisal of a century of research in parapsychology can, I think, conclude that parapsychology is anything less than it purports to be: a solidly empirical investigation of certain anomalies in our psychological makeup that we call, for want of a better term, "psychical abilities."

The Beginnings of Parapsychological Research

The early period of psychical research, dated from 1882 with the founding of the Society for Psychical Research, was closely associated with the nineteenth-century spiritualist movement. Psychical researchers were concerned with the rampant fraud and trickery that were practiced by many, if not all, mediums. But if survival after death could be proven beyond doubt, this would have profound implications for philosophy, religion, and science, as well as the concerns of daily life. Therefore the reputable philosophers and scientists of the Society for Psychical Research felt a need to investigate mediumistic communications in the hope of separating the veridical phenomena, if such existed, from the fraudulent.

The investigations of the Society for Psychical Research were conducted with rigid controls and under conditions that seemingly excluded all possibility of fraud. Some of their investigations did produce evidence strongly in favor of survival after death. But the problems were only beginning. Even among psychical researchers the survival hypothesis—that returning spirits of the dead were at work—was challenged by other explanations: clairvoyance, telepathy, and coincidence. And even when the conditions of clairvoyance and telepathy seemed to have been excluded, the possibility of coincidence remained. Just how striking, unusual, or improbable must a coincidence be before one accepts the even more improbable explanation that a paranormal form of communication is involved?

The events of our everyday life are determined by so many interacting variables that it is difficult to specify exactly how often two independent events will be associated by chance alone. For example, how often might it occur that a person A will dream of a person B's death on Tuesday night and learn Wednesday morning that B did indeed die during the night? To objectively evaluate the hypothesis that this dream presented a paranormal event one would have to know: (1) how often A dreams of B's death, when B does *not* die, (2) whether B's death was highly probable in its own right, and (3) whether A had access to any information through normal sensory channels—such as overhearing a conversation, later forgotten, or awareness of body language or faint chemical stimuli (pheromones) that might indicate that B was in poor health—which might have acted as a subliminal stimulus, giving rise to the dream. To provide evidence of a paranormal event, it would

also be essential for A to have communicated the dream to a third party (preferably in writing, noting date and time) before being apprised of B's death.

The need to bring paranormal events into the laboratory where they could be observed under controlled conditions and systematically explored led to the establishment in 1927 of the Parapsychology Laboratory at Duke University by William MacDougall and J. B. Rhine. Concerned with the need to evaluate statistically the probability of occurrence of psychic events in the laboratory, the first task of these men and their colleagues was to find a population of discrete events for which the probability of chance occurrence could be fixed objectively, and then compared to the observed value when purportedly paranormal influences were operating.

The Zener cards used in many early investigations at the Duke Parapsychology Laboratory are an example of such a population of discrete events with fixed probability. These cards consist of five each of five symbols (circle, square, cross, star, and waves). When each card is placed face down on the table, and a subject is asked to guess what the unseen card might be, the subject has a one-out-of-five chance of being right. Thus scores for pure guessing in such a clairvoyance task would hover around the twenty percent level. An occasional score falling more than two standard deviations beyond mean chance expectation would also be expected on the basis of chance alone, but these occurrences would tend to be infrequent (falling as they do at the tail ends of the normal curve) and the circumstances surrounding their occurrence would show no consistent pattern. However, if scores significantly different from chance occurred repeatedly over many trials, and furthermore seemed to be lawfully related to situational or personality variables, then one might hypothesize that some non-chance factor was influencing the results. It is just such a pattern of non-random interactions in the body of ESP data collected to date that has convinced many observers that the factor called "psi" is manifesting itself.

By 1934 Rhine and his associates had used the ESP cards in over 90,000 trials with gifted subjects. They were reluctant to publish the results of these studies until they had established to their satisfaction that the astronomically high levels of extra-chance scoring obtained with their subjects were not just a fluke, but were indeed repeatable over many sessions and could be verified by independent observers. Having refined his experimental techniques and subjected the data to rigorous statistical evaluation, it was not until 1934 that Rhine finally published his monograph *Extrasensory Perception*.

This volume described, among other work, the now famous Pearce–Pratt series of experiments. Pratt, then a graduate student at Duke, served as the experimenter, conducting trials at fixed intervals by means of synchronized watches while Pearce, as subject, was in some different building from 100 to 250 yards away. At intervals of one minute, Pratt would take a card from the

top of the deck and place it face down on the desk in front of him. Pearce, in the library building across campus, would record his guess. Then the procedure would be repeated. Pratt did not look at the faces of the cards until all 25 had been used as targets. He then recorded the sequence of the 25 target cards, placed his record in a sealed envelope, and delivered it immediately to Dr. Rhine. Pearce, in the meantime, was on his way back from the other building with the sealed record of his guesses. Over the whole series of 1,850 clairvoyance trials run under varying conditions of distance and experimental controls, Pearce obtained 558 "hits" where only 370 would be predicted by chance. He averaged 8.3 correct per run of 25 cards, where chance alone would give an average of 5 correct.[2]

Another experiment described in Rhine's 1934 publication was the Ownbey–Turner series, which was conducted to determine whether physical distance would interfere with telepathic rapport. Over a period of eight days, Miss Ownbey, at the Duke Parapsychology Laboratory, attempted to "send" 25 cards at five-minute intervals to Miss Turner, who was 250 miles away. Twenty-five calls were made daily, with both agent and percipient keeping separate records. Miss Turner's records were delivered to Dr. Rhine, who independently checked them against Miss Ownbey's records of the actual target cards. The results for the first three days were extremely successful with 19, 16, and 16 hits where chance would predict a mean score of 5 correct calls out of 25. Over the next few days the scoring rate declined somewhat, but the overall scoring rate averaged 10.1 per 25 for the 200 trials. Scoring was as good, or better, at a distance of 250 miles as when agent and percipient were in close proximity to each other.

The early attempts to determine the conditions that affect the manifestation of ESP included an investigation of drugs that affect central nervous system arousal level. An experiment was conducted in which George Zirkle, the subject, was administered five grams each of sodium amytal and caffeine in identical capsules. Although the agent/experimenter (another graduate student) was not blind to which drug was being given at which time, Zirkle was unaware of the order in which the drugs would be administered, and neither experimenter nor subject had any knowledge of the kind of effects the drugs might be expected to have on ESP scoring. With the agent and percipient in separate rooms approximately 8 to 12 feet apart, a baseline series of 50 trials drug-free was made on the same day as the drug experiment. In this baseline condition, ESP scoring averaged 13.5 hits out of 25 cards. Then five grams of sodium amytal (a depressant) in a blank capsule was administered to Zirkle. One hour later he was very sleepy and his ESP scoring rate declined to 7.8 per 25. Another test was made two hours later. At this point Zirkle had to be physically kept awake, was having difficulty walking straight, and was experiencing impairment in vision and memory functions; his ESP scoring declined further to 6.2 per 25. At this point Zirkle was administered five

grams of caffeine. When tested one hour later he was much more alert and his motor coordination had returned; his scoring also returned to 9.5 hits out of 25 trials. Thus ESP scoring rates seem to reflect the effects of depressant and stimulant drugs on the central nervous system. Drug tests with other experimental subjects followed a similar pattern.

Psychological patterns began to emerge from the data. Scoring would be above chance when the subject's interest or motivation was high, but would decline as the subject became bored or fatigued. Physical illness would also cause scores to fall to chance level or below. ESP scoring seemed to be highly influenced by subjects' attitudes and expectations. In early tests of the effects of distance on telepathy, subjects tended to believe that ESP could not work at distances over a few feet. Scores thus tended to show a decline with distance. However, when the task was presented to the subjects as a challenge, ESP scores were often better using screens or separate rooms at some distance than they were in the same room. Thus physical distance seemed to be less important than "psychological" distance.

Once the explorations of clairvoyance and telepathy[3] were well under way, Rhine (1938) reported the results of the first experiments on precognition using a technique known as the "psychic shuffle." Subjects were given a response sheet with 25 blanks and asked to write in their guesses of the order in which the 25 cards in the ESP deck would occur after they were shuffled. The catch was that this shuffling had not yet taken place, so the subjects were in the position of having to predict the order of future events. Preliminary results using this technique gave odds of less than one in a million against a chance explanation of the results. However, problems of interpretation soon arose. Perhaps precognition, with its implied violation of temporal causal relations, was not the most parsimonious interpretation of these results. Possibly while the experimenter shuffled the cards, he might have used clairvoyance to determine the precise instant at which the cards approximated the order of the subject's previous responses, and then conveniently stopped shuffling. Alternatively, perhaps while shuffling the cards, the experimenter used psychokinesis (PK) to cause them to take on the sequence of the subjects' calls. Ensuring that experimenters were blind to subjects' responses until after card order had been determined, and introduction of a mechanical device to shuffle the cards, eliminated the clairvoyance hypothesis, but still left the mechanical device open to PK action.

In an attempt to counteract the possibility of PK influence on the card shuffler, an automatic device was incorporated that would randomly cut the cards after the shuffling process was completed. With this modification, precognition scores declined to the chance level. However, closer inspection of the distribution of the scores revealed a curious non-random pattern: a concentration of hits at the beginning of the run, with a gradual decline (hence the name "decline effect") and a brief flurry of successes at the very

end of the run. This pattern, which has been found over and over again in psi scores from many different experiments and researchers, is one of the most consistent patterns to emerge from psi experiments. The occurrence of the "decline effect" in the precognition data was interpreted as showing a high initial level of psi, reduced by fatigue and declining interest and motivation, attributable to what is essentially a monotonous and repetitive task. Thus the presence of a decline effect in the subjects' precognitive guesses implied that some non-random factor was indeed operating, and this was taken as evidence for the existence of precognition. Later work from the Duke Parapsychology Laboratory gave results that were less difficult to interpret (e.g., Anderson, 1959).

For many people the ability to influence what is going on in someone else's mind, or to obtain information clairvoyantly, may not be as hard to accept as the thesis that it is possible to gain information about future events before they occur. However, just as distance in space does not appear to affect clairvoyance or telepathy (Osis, 1965), neither does distance in time appear to affect success on a precognition task. Anderson (1959), at the Duke Parapsychology Laboratory, worked with a subject who mailed in her ESP responses from Paris, France. Half of the subject's ESP responses were scored against targets selected immediately (which was in fact a few days after the distant subject had made her calls). The other half of the subject's ESP calls were scored against targets that were not selected until one year later. Although the targets selected immediately gave chance results, scores from the year-interval test were independently significant, with odds of 500 to 1 against chance. Scores in the two conditions taken together were also significantly above chance.

Further work from the Duke Parapsychology Laboratory concentrated on the possibility that the outcome of a random throw of the dice might be affected by the human mind. Preliminary tests with a young gambler who claimed to be able to influence the fall of the dice inspired Rhine to develop a more formal test of the phenomenon. Using hand-thrown dice, Rhine had 25 subjects make 24 throws each. Results were significantly above chance, with a probability of over a billion to one (Rhine and Rhine, 1943). More controlled conditions were soon implemented, including the use of a mechanical dice-thrower and corrections for imperfections in the dice that might cause one face to come up more often than the rest. Also, if the subject was allowed to determine for himself the target face for each trial, his later success could be interpreted as precognition of the outcome. To control for both these possibilities, the choice was taken out of the subject's hands, and target faces were assigned in a predetermined random order with an equal number of trials for each face. The PK ability of varius subjects seemed to be unaffected by these variations in technique, and overall probability values as high as $p =$

.008 were obtained (that is, eight chances in a thousand of getting such a result, if only chance factors are operating) (Rhine, 1943).

Skeptical Objections and Methodological Considerations

The first reports from Rhine's laboratory were greeted skeptically in many quarters. The early criticisms of laboratory psychical research centered on the following possibilities:

(1) Subjects might have had an opportunity to cheat while left alone with the targets.
(2) Some form of normal sensory cuing (such as markings on the targets, or slight variations in the experimenters' facial expression, body language, or tone of voice) might account for the extra-chance scoring levels, with no extrasensory information transfer involved at all.
(3) Non-randomness in the target sequences might coincide with a subject's response bias tendencies, yielding a spuriously above-chance score.
(4) The experimenter(s) might have inaccurately recorded the subject's responses.
(5) Data analysis might have involved inappropriate use of statistical tests.
(6) In order to prove that ESP exists, the experimenter(s) might have deliberately fabricated the evidence.

These methodological criticisms prompted further refinement of experimental techniques. Current parapsychological methodology emphasizes the need to incorporate rigid controls in order to exclude all explanations other than the operation of ESP. The following precautions have become standard methodological procedure in experiments accepted for publication in the reputable journals of the field.

1. Strict procedures are followed to ensure that the subject has no opportunity to cheat. The subject is never left alone with the targets in a clairvoyance experiment, and is not allowed to communicate with the agent in telepathy experiments. Targets are concealed from the subject by a screen or an opaque envelope, or better yet, they are kept in a distant place to which the subject has no access.

2. In order to eliminate sensory cues, targets are handled as little as possible, so that random scratches or markings do not become the basis for the subject's responses. The targets are prepared by an independent assistant who has no contact with the subject. Thus the experimenter who presents the sealed targets to the subject is "blind," that is, has no knowledge of which target is in which envelope, and cannot systematically influence the subject's response in the direction of a higher score.

3. Elaborate procedures are followed to ensure random selection and presentation of targets. Random number tables or other random sources (REG determination, or computer algorithms) are the source for target

sequences, and targets or conditions are presented to the subjects in a counterbalanced sequence.

4. Scoring must be double-checked later by an independent scorer blind to the hypothesis of the experiment, and ignorant of the experimental group to which the subject belongs. Alternatively, two observers can make simultaneous but independent records of the subject's responses and later compare them for errors in recording.

5. The question of the appropriateness of the statistics used to evaluate ESP data was submitted to the American Institute of Mathematical Statistics for evaluation. In 1937 the president of that organization issued a public statement that verified the adequacy and integrity of the statistical techniques used by Rhine and his colleagues at the Duke Parapsychology Laboratory (Camp, 1937).[4]

6. The charge of experimenter fraud has often been made against parapsychological data as a kind of last resort, after all other criticisms have been answered or invalidated. Regrettably, however, the charge of experimenter fraud has not been without validity in some few cases, notably those of W. J. L. Levy and H. B. Soal (see the paper by Rhine in this volume [Rhine, 1974], and Markwick, 1978). Although fraudulent manipulation of data may have occurred on only one occasion in these investigators' research careers, that is sufficient to bring into question all of their published research. Parapsychologists are generally among the first to repudiate those among their ranks who are found guilty of such unethical practices, first because this represents unacceptable procedure in science, but perhaps also since these occurrences cast doubt on the integrity of all other researchers in what is already a highly controversial field. But does the fact that two experimenters have been found guility of fudging data mean that *all* the published evidence for the existence of ESP is fraudulent? To contend that, over the decades, scores of highly respected researchers with otherwise impeccable reputations have conspired in a gigantic hoax to falsify evidence and deceive the public and their fellow scientists is even less parsimonious than the alternative hypothesis: that ESP exists.

To summarize the crucial elements of methodology: no matter what the form of psi being investigated, double-blind methods are essential. That is, the assistant who prepares the targets has no contact with the subjects. The experimenter who interacts with the subjects does not know what the targets are on any given trial. Scoring is double-checked by an assistant who is blind to the hypothesis of the experiment, had no contact with the subjects, and does not know to which experimental group or condition the subjects belonged.[5] Finally, it is important in parapsychological research, as in other fields of scientific endeavor, to await independent replication of a finding before drawing any definite conclusions.

In the "hard" physical sciences, the criterion of the repeatable experiment is interpreted to mean that anyone can follow the same experimental procedures and get the same results every time. ESP experiments, on the other hand, tend to be frustratingly inconsistent. A novel procedure will often give highly significant results when it is first introduced, only to seem to "fail" later on, when results decline to chance, or when the effect reverses direction completely. If psi is indeed a natural and lawful phenomenon, it should be consistent and predictable once one can specify the laws and conditions that govern its occurrence. To many scientists, this lack of repeatability in psi experiments is the most powerful argument against the existence of psi.

But this argument may be somewhat too extreme (Rhine, 1959). The repeatability of psi experiments is actually higher than is generally assumed. Certain findings have been demonstrated experimentally over and over again. Take, for example, the effect of belief in ESP on scoring levels (the "sheep-goat effect," reviewed by Palmer, 1971); or the positive effects on ESP scoring of high interest and motivation, and the negative effects of withdrawal, apathy, and negativism (Nicol and Humphrey, 1955; Rao, 1962; Scheerer, 1948; Shields, 1962). On the other hand, it is incorrect to assume that the repeatability criterion is met by all fields of scientific endeavor *except* parapsychology. Epstein (1980) points out that the social or "soft" sciences in general (including many fields within psychology) experience difficulties in replicating experiments. Epstein suggests that this problem may be inherent in the nature of the sampling assumptions within the experimental method itself. If repeatability in psychology in general is less than perfect, the problem in ESP experiments is even more pronounced. Psi scores have been shown to be affected by the subject's personality, attitudes, mood and motivation, and the nature of the subject's social interaction with the experimenter and the testing situation. Experiments in which these variables are not taken into account generally show chance results. The high variance in the data caused by the effects of these uncontrolled variables may in many cases obscure the operation of the underlying psi processes.

Current Techniques in Parapsychology

The combined weight of experiments such as those described in the first section—including the weight of much more recent work with progressively tighter methodological controls—is strongly against a "random-chance" explanation. As it became obvious that merely demonstrating the existence of non-random scoring tendencies in the data would not yield any immediate understanding of the psi process (or processes), interest turned to a search for the conditions under which psi was manifested and for the kinds of personality attributes that correlated with psi success (see, e.g., Carpenter, 1977; Honorton, 1977; Palmer, 1977). As reseachers became dissatisfied with

repetitious card tests that failed to hold subjects' interest, new instrumentation and more sophisticated experimental methods were introduced.

One of the recent developments in parapsychological research has been the attention given to the importance of an alteration in the normal state of consciousness in order to make the psi signal "heard" above the "noise" of the other sensory stimuli against which it must compete. Honorton (1977) has provided a complete review of all major research dealing with alterations in state of consciousness in the direction of "internal attention states."

Relaxation is one of these states that has been quite extensively investigated as a psi-conducive technique. For example, L. W. Braud and Braud (1977) conducted a clairvoyance experiment where 100 subjects were to attempt to describe art prints in sealed envelopes. Subjects listened to a tape of suggestions selected to promote relaxation, calmness, feelings of coolness in the forehead, and heaviness and warmth in the extremities. The tape was designed to test the theory that a reduction in arousal of the autonomic nervous system would facilitate psi scoring. Apparently the tape was successful, because 36 of the 100 subjects made direct hits (first choice on ranking the possible targets) when asked later to identify, from among several art prints, the one that had been their concealed target ($p = .0055$). In total, 63 of these 100 subjects placed the actual target in the top half of their rankings of the art prints ($p = .0047$).

Earlier research by the same team of Braud and Braud (1974) reports two studies using the same tape of relaxation suggestions in a telepathy (GESP) paradigm. In the first study, 16 subjects listened to the same tape-recorded instructions. Subjects recorded their impressions of the ESP target and then ranked their degree of relaxation on a ten-point scale. Highly relaxed subjects scored significantly better on the free-response GESP task than did less relaxed subjects ($p < .04$; that is, less than 4 chances in 100 of this result occurring by chance alone). In the second study, involving 20 subjects, 10 of the subjects listened to the relaxation tape, while the remaining 10 heard a tension-inducing series of instructions. The relaxation group showed significantly more psi success on the ESP task than did the tension group ($p < .025$). The degree of relaxation was monitored by EMG recording devices (though no feedback was given) and the degree of both objective (EMG) and subjective (self-rated) relaxation correlated significantly with psi success (correlations of .49 and .53 respectively, $p < .05$ in both cases).

In line with the hypothesis that reducing external sensory distractions and redirecting attention internally will facilitate psi, a large number of studies involving the ganzfeld technique have been reviewed by Honorton (1978). The ganzfeld is a mild form of sensory deprivation where the subject sits in a comfortable reclining chair, with eyes covered by screens that exclude patterned visual stimulation (usually ping-pong balls cut in half and taped

over the eyes), with the visual field flooded with red light. The subject simultaneously listens to a tape of white noise. Thus patterned stimulus input is totally excluded and the subject can turn his attention inward and report his ongoing mentation into a tape recorder. An agent sits in another room, without contact with the subject, and views a target picture while trying to "send" information about the picture to the subject (percipient). At the conclusion of the session, the percipient is shown several pictures, one of which was the actual target, and he is asked to rate the pictures for the degree of correspondence to his ongoing imagery and associations.

The ganzfeld has proven to be one of the most successful techniques for eliciting psi and has stood up under the test of numerous replications. Seven of the eight studies conducted in Honorton's lab were independently significant, giving a combined significance level of 7.9×10^{-8}. Honorton demonstrates in his review that of 23 studies from 11 different laboratories (not including his own), 14 studies demonstrated overall significant psi scoring rates. This constitutes a replication rate of 54 percent where only 5-percent-successful replications would be expected if the results were just flukes caused by chance variations of the normal curve.

A major methodological advance over the early work with ESP cards and dice was made when Helmut Schmidt, an electronics engineer by profession, developed a series of automated machines known as random event generators (REGs). These devices are based either on radioactive decay processes (the most completely random processes yet discovered) or on random electronic-noise sources. The Schmidt machines operate by generating random binary electrical pulses that serve as targets, in the manner of an electronic coin flipper: a positive pulse is "heads" and a negative voltage is "tails." The Schmidt REGs have several advantages over earlier experimental techniques: (1) they are completely random, so that their target sequences do not suffer from the problem of "pseudorandomness" inherent in printed random number tables or in computer algorithms; (2) they allow for automatic target and response recording, thus excluding the possibility of experimenter error in recording the results; and (3) they provide immediate feedback to the subject on whether his guess was right or wrong, thus allowing for experimental study of the application of operant conditioning learning techniques to ESP (see Tart, 1976).

One of these random event generators was used by Schmidt (1969*a*) in a clairvoyance test to compare the responses of six subjects to a prearranged random target sequence. Over 15,000 trials, the results of these tests were highly significant, with odds of over a million to one against chance. The Schmidt REGs have also been very useful in studying precognition. The subject makes his response a fraction of a second before the random target is electronically generated; thus many trials can be accumulated over a short period. Schmidt (1969*b*) used his random event generator to test precognition

with three gifted subjects. Over a total of 63,066 responses, the results from these three subjects were highly significant, with odds of 2,000 million to 1 against chance.

In another experiment also using one of the Schmidt REGs, large numbers of subjects were tested in groups. Five hundred trials were run in both precognition and PK test modes, and results were significantly above chance in both cases (Schmidt and Pantas, 1972). In Part 2 of this same experiment, a single subject meditated for approximately twenty minutes before making a series of either precognition or PK calls. With 500 trials in each mode, extreme deviations from chance expectation were found in both conditions, and no difference in scoring rate was found between the precognition and PK conditions. This overlap between the PK and precognition abilities has profound theoretical implications: it may be that we do not merely gain "knowledge" of our future, but actively create it.

Along with experiments on dice and electronic random event generators, much recent PK work has used natural or biological systems as targets. Interest has focused on whether the reports of "psychic healing" might not be founded in a PK ability to influence living systems. Thus Smith (1972) had a noted psychic healer (Mr. Estebany) hold a sealed flask of enzyme solution (trypsin), and found significant increases in the rate of enzyme catalysis when the flask was held by the healer, as opposed to the rate in a control (untreated) condition. To control for the effects of temperature on trypsin activity, a sensitive thermistor was placed between Mr. Estebany's hands as he held the trypsin sample; the water bath in which the control sample was placed was kept at the same temperature. Replications of this experiment have been moderately successful (see Edge, 1980); the limited success is attributed to the difficulty the healer has in establishing a caring personal relationship with a flask of enzyme solution. In Smith's experiment, a control condition where identical flasks of enzyme solution were exposed to a high-intensity (1,300 gauss) magnetic field for three hours showed definite results in the same direction as, but stronger than, the effects produced by the healer. It is therefore possible that psychic healing produces its effect by inducing a magnetic field; however, using a magnetometer, Smith found no indication of any unusual magnetic field around Estebany's hands while he was "healing" the enzyme solution.

Grad (1965, 1976) has worked with the same healer who claimed to be able to heal through the laying on of hands. In this work, Grad had available to him the facilities of a biomedical research laboratory with large numbers of experimental animals (mice). For the psychic healer to administer the "treatment," mice were placed into a specially constructed iron cage with a solid bottom and a wire mesh top. The healer held one hand below the cage and the other one covering the top of the cage. At no time did the healer actually touch the mice themselves in any of the experiments. In control

conditions, mice were exposed to warm temperatures by placing a heating pad under their cages for an equivalent amount of time.

In one experiment, mice were fed an iodine-deficient diet that produced large goiters in two control ("untreated") groups of mice, whereas mice whose cages were held by the healer showed significantly less goiter development. In another experiment, which has been successfully replicated several times, equal-size patches of skin were removed from the backs of 300 mice. Mice whose cages were held by the healer showed a much faster rate of wound healing than did the control animals ($p < .01$). Mice whose cages were held by skeptical medical students healed the most slowly of all three groups.

This PK explanation of psychic-healing phenomena has also been studied with plants. A state of "need" in the plants was induced by treating the seeds with a one percent sodium chloride solution. Salt treatment damages the seed and tends to inhibit sprouting and subsequent plant growth. However, seeds treated with saline solution that had been previously held by the healer for fifteen minutes showed significantly more subsequent growth than control seeds treated with solution not held by the healer. A chemical analysis of the hand-held saline solution revealed no difference between the hand-held and the control saline solutions in sodium concentrations or pH value.

In another experiment along these lines, Watkins and Watkins (1971) tested the ability of nine reputedly psychic subjects to hasten the resuscitation of anesthetized mice. Five of these subjects claimed to have healing abilities, and eight of the nine scored well on laboratory PK tests. On the average, the experimental mice (those concentrated on by the psychic subjects) awoke in 87 percent of the time taken by the control mice. Eight of the nine psychic subjects were able to induce a significantly shorter duration of anesthesia in their mice, whereas none of the three control subjects could produce this effect ($p < 10^{-5}$).

Another experiment, conducted by Schmeidler (1973), attempted to determine whether the effects of PK could be voluntarily controlled and localized in space. This time, instead of using living systems as targets, a talented psychic attempted to influence temperature-sensitive devices (thermistors) which were sealed into vacuum bottles and connected to an automatic recording device. The subject was asked to raise and lower the temperature of specific thermistors on demand, following a predetermined, counterbalanced sequence. The subject was successful at producing temperature changes in the desired direction over repeated trials. Sixteen trials made up an experimental session, and in seven out of ten sessions the temperature deviations of the sixteen trials were statistically significant. In five of these seven sessions, the probability was less than one in a thousand of chance fluctuations being responsible for the results. Curiously, a drop in the temperature recorded by one thermistor was often accompanied by a rise in air temperature in a different part of the room, as if the heat energy had

somehow been transferred from one part of the room to another. The results suggest that PK may operate in accordance with the laws of thermodynamics.

Conclusion

This brief review has only scratched the surface of the issues in collecting and interpreting parapsychological data. There is a great deal of evidence, which it has not been possible to review here in detail, that the psi process bears a lawful relation to personality and attitude, interest and motivation. Psi seems to be related to a host of other psychological variables. Spontaneous cases show that ESP may lie dormant for many years, a small voice that is not heeded above the demands of coping with everyday life, until a moment of overwhelming personal crisis. It is not possible to duplicate in the laboratory the crisis situation and the intense emotional involvement evident in spontaneous cases. Thus ESP experiments offer at best a shadow of the true nature of psi. And yet the scientific method has had great success in unraveling other mysteries of nature. Can it not be fruitfully applied to mysteries of human nature as well?

Of course, any experimental evidence is only as good as the conditions under which it was obtained. This is the reason for the meticulous attention to details of methodology in parapsychological research, given the importance of what we are trying to demonstrate.

If one accepts the evidence as valid, then how is it to be interpreted? At least four different aspects of psi functioning have been identified (clairvoyance, telepathy, precognition, and psychokinesis). But curiously they often seem to occur in the same individuals, and to be influenced by the same conditions. Once the existence of these discrete forms of psi had been demonstrated, it was no longer possible to interpret unequivocally the results of any ESP experiment in terms of only one psychic ability. The evidence in support of precognition using the "psychic-shuffle" technique could be explained, with somewhat less violation of causality in temporal relations, as clairvoyance by the card shuffler, or alternatively as PK effect on the cards or on the mechanical card-shuffling device. Psychokinesis results could be interpreted as involving precognition in the correct call of the die face before it actually came to rest. Telepathy could be seen either as unassisted clairvoyance on the part of the percipient, or conversely as a PK process exerted by the agent on the percipient's brain cells, causing him to "perceive" the correct answer.

These overlapping theoretical explanations of psi phenomena give rise to a feeling that psi phenomena might not actually be discrete classes of events, involving different processes and mechanisms. Instead, psi ability might be a unitary phenomenon. Our thinking about the different aspects of psi may be more a consequence of the different methods we use to elicit them than an

indication of any fundamental differences in the nature of the psi process itself.

Those who have tried to theorize about the nature of the psi process have taken many different approaches. At first the search centered on physical conditions and variables. But tests with Faraday cages (Vasiliev, 1963) have ruled out most of the frequencies of the electromagnetic spectrum as "carriers." Psi does not obey the inverse square law; thus one cannot speak of it as a kind of "mental radio." Psi appears to be independent of space and time; therefore no presently known form of physical energy can account for the observed data. Speculation has turned to the esoteric fringe of contemporary physics (Oteri, 1975). Paradoxically, some theorists in quantum mechanics do not seem to feel that ESP violates any physical laws at all!

Building on a quasi-physical approach, Murphy (1945) and Roll (1966*a*) have speculated on the existence of interpersonal "psi fields." More within a mainstream psychological tradition, Roll (1966*b*) has proposed that psi information may be mediated by the personal memory structure. Stanford has developed a theory relating to unconscious motivation (1974*a,b*). W. G. Braud (1975) described a "psi-conducive syndrome" characterized by relaxation and imagery, and Honorton (1977) has emphasized the importance of "internal attention states." These theories are based on a noise-reduction model, which allows the "weak" psi signal more access to processing capacity (Irwin, 1978*a,b*).

Yet the final word has not yet been uttered in these debates. As the human race begins to explore outer space, this aspect of our inner space still remains a mystery. We search for other forms of intelligent life elsewhere in the galaxy, but we have not yet fulfilled Socrates' command to "Know thyself."

Notes

1. A brief note on terminology seems in order. ESP consists of telepathy (mind-to-mind communication) and clairvoyance (obtaining information that is not known to another human mind). Precognition refers to gaining knowledge of the future, and psychokinesis (PK) refers to physical effects of the "mind-over-matter" variety. Collectively these various abilities are known as "psi."

2. These experiments, described in further detail by Rhine and Pratt (1954), have been criticized by Hansel (1966) on the grounds that under the experimental conditions described, it was hypothetically possible for cheating to have occurred, and that the experiments were not sufficiently stringent in their supervision of the experimental subject Pearce. Hansel's charges were refuted by Rhine and Pratt (1961) and again by Pratt (1969). The reader who wishes to make up his or her own mind is referred to this very interesting exchange.

3. In spite of the evidence from his own laboratory which proved the existence of ESP, Rhine (1945, 1974*a*) has questioned whether telepathy actually exists. He notes that most experiments on telepathy (mind-to-mind communcation) are carried out under conditions where the percipient's clairvoyance (ESP not involving information

gained from another human mind) could actually be responsible for the results, with the agent's contribution being superfluous or irrelevant. Rhine lists several criteria that must be met to demonstrate conclusively that pure telepathy exists. Rhine's main requirement is that targets cannot be written down, spoken aloud, or exist in pictorial form, or in any other form amenable to clairvoyant perception. The only record of the target that can exist is a purely mental impression. McMahan (1946) and Birge (1948) have made ingenious attempts to meet these requirements, but the evidence for pure telepathy remains inconclusive. Along with Rhine, most parapsychologists today regard this issue as an "untestable hypothesis" and are content to allow for the possibility of clairvoyant perception in telepathy experiments. In recognition of the difficulty in distinguishing between the two, telepathy is commonly referred to as GESP (General Extra-Sensory Perception).

4. This vindication has not prevented the same criticism from being raised on subsequent occasions (see, for example, Hansel, 1966). When it comes to evaluating the occurrence of chance and coincidence against other, less parsimonious hypotheses (such as the occurrence of ESP), the need for parapsychologists to have a thorough grounding in the mathematical assumptions and techniques of statistics cannot be overemphasized.

5. In addition to these standard methodological considerations, special precautions are required depending on what form of ESP one wishes to investigate. Space does not permit more detailed coverage of the special methodological considerations for telepathy, precognition, and psychokinesis, but the interested reader will find a more in-depth discussion of these issues in Schmeidler (1977).

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Coincidence and Explanation

GALEN K. PLETCHER

Skeptics sometimes dismiss putative paranormal occurrences as "coincidences." A seemingly clairvoyant dream, for example, will be partly explained by the usual mechanisms of dream formation, and the residue will be dismissed by a general waving of hands and the attribution of coincidence. Persons sympathetic to belief in paranormal occurrences fume against this kind of treatment, sometimes seeing in it a kind of unreasoning intransigence not unlike that which plagued Galileo. I wish to examine some features of coincidence in this paper. I believe that the attribution of some phenomenon to coincidence is a more serious claim than is usually appreciated, even though if not carefully defended it can indeed be an irresponsible dismissal. My general position, only part of which is developed in this paper, is that in the absence of improvement in consistency in the results of laboratory investigation of ESP phenomena, nothing short of a new (or drastically revised) explanatory framework will provide a *rational* basis for deciding that a seeming coincidence has any more significance than the skeptic sees in it (namely, none).

To call something a "coincidence" explains nothing. It does exactly the opposite: it asserts that the fact that two events are closely related—in time, and in other ways—does not need to be explained. It says more than that the relation between them cannot (at present) be explained, for if we felt that the failure of an explanation were only a matter of our (always changing) grasp of the external world, we would not call it a "coincidence." Of course we are sometimes mistaken in taking some phenomenon to be a coincidence. But then, on learning more about the situation, we no longer call it "coincidental."

My thinking about these matters has been greatly helped by conversations with several of my colleagues at Edwardsville, although they are not to blame for faults that may remain. I am particularly grateful to Professor Ronald J. Glossop for sustained and painstaking commentary, both oral and written.

It follows that to call something a coincidence is to express (even if only implicitly or perhaps even unwittingly) the opinion that it is misguided to search for an explanation (in the proper sense) of the coinciding of the phenomena at issue. It expresses the view that the explanations for each of the occurences will not involve reference to the other; and furthermore, it implies that there is not some level of explanation that will ultimately account for both of the phenomena. This is a very great deal to say. In fact, it might occur to us to wonder whether we ever are in fact justified in calling some set of events a coincidence.

We will find, I think, that most claims that two events are coincidental have reference to some suggested possibility about the relation of the events. If we say that a putatively clairvoyant dream is only coincidentally related to the event or state of affairs seemingly related to it, we do not mean that there is *no* connection between them, for the very good reason that this claim would usually be false. A putatively clairvoyant dream about some relative of mine gets at least part of its material from my knowledge of and acquaintance with that relative. If I express the opinion that it is a coincidence that the circumstances I dreamed about do in fact obtain, I do not mean to say that there is no connection between the fact that my cousin is a Caucasian and the fact that the person in my dream was also a Caucasian. But I do mean to deny, when I say that the whole thing is a coincidence, that there was a relationship between that state of affairs and my dreaming of it, such that an explanation for the occurrence of one would involve reference to the other.

It is at this point that disagreement arises between the skeptic and the person who would allege that the dream is in fact clairvoyant. The latter maintains that there *is* some relationship between the state of affairs and the dream. Moreover, the claim usually made is that the relationship is (somehow) causal, and that the dream could therefore be a legitimate basis for a knowledge claim concerning the state of affairs dreamed about. That a knowledge claim is usually not made would be attributable, according to those arguing for clairvoyance, to the fact that the dream is not ordinarily *taken* as a basis for a knowledge claim, and hence no belief is entertained on its basis. When the existence of a correspondence *is* discovered, it is on the basis of some better-established way of gaining knowledge, and we can then claim to know the state of affairs on that latter basis alone.

The "believer" in this little story will attribute the concurrence of dream and reality to clairvoyance. To what extent does that explain the phenomenon of resemblance between state of affairs and dream? It seems to offer no explanation at all, but instead to suggest a classification of the phenomenon in one of many possible ways. To call a phenomenon "clairvoyance" assimilates it to a group of such phenomena in which (1) the *usual* modes of perception do not seem to be in effect, but (2) we do not wish (for one or more reasons) to consider the resemblance a coincidence. In this

way, "clairvoyance" and other names of ESP phenomena are (as parapsychologists J. B. Rhine and J. G. Pratt have recognized) "mere descriptive terms that were applied to the phenomena as they came to be identified in the early stages of the developing science" of parapsychology (J. B. Rhine and Pratt, 1957, p. 9). Such a naming operation brings us no closer to understanding the congruence of dream and state of affairs than we are when we call them a "coincidence." But there is one crucial difference: to attribute the phenomenon to clairvoyance is to claim that the fact that the two events are closely related—in time, and in other ways—*does* stand in need of explanation. It is to claim, moreover, that to treat them as if they were two discrete and unrelated occurrences would be to miss some of the relationships that are to be found in the world.

But we do not know, at this point, what such relationships are. It may be that we can agree that to call a phenomenon "parapsychical" (or some synonymous term) is to say of it that it "has been shown by experimental investigation to be unexplainable wholly in terms of physical principles" (J. B. Rhine and Pratt, 1957, p. 6). But we do not know of it, as Rhine and Pratt go on to claim, that it is "a mode of perception that is independent of the senses" (p. 7). At least, we do not know this so long as our means of identifying such phenomena is that they are not accountable by means of the physical principles known to us. We can see this by means of an example.

A student sees the papers and sundry notes on my bulletin board, and suddenly becomes very interested in their "order," as he calls it. I say, in response to a question, that they are in no particular order, but that I have tacked up various bulletins, memos, calendars, and cartoons as seem to me noteworthy. He persists, and eventually it emerges that the colors of those papers on the bulletin board that are other than white are arranged alphabetically left to right. They are arranged, let us say, in the order of blue, gold, green, pink, and yellow. He now presses me to explain how they got that way. I will explain to him that this blue sheet is the official listing of University holidays and, because of its size, fits just above this Pogo cartoon, while this green sheet was put here because . . . and so on. I will utilize explanations entirely unrelated to the colors of the sheets of paper. He, however, will press for explanations having *reference* to their colorations. For me, their color arrangement—that one feature of their configuration on the bulletin board—is coincidental. Once I have explained how I came to place each individual sheet where I did (relying on when they were received, their importance, their sizes, and the like), I will be satisfied. He, however, will urge that such an explanation of the placement of the sheets cannot fully explain their color configuration. He will maintain that it couldn't just have *happened* that they are so arranged, whereas I will maintain that it not only *could*, but it *did*, "just happen."

The difference in the two positions just outlined is that the student regards a certain feature of the arrangement of the papers on my bulletin board as standing in need of an explanation additional to those I have given, whereas I regard this same feature of the arrangement of papers as *derivative* from the explanations I have given, and not requiring additional explanation beyond those. When we try to evaluate the claims of spontaneous clairvoyance, and of other paranormal phenomena, we are in a very similar situation. If X has a dream in which a state of affairs is featured in some detail, and then later encounters a remarkably similar state of affairs in waking life, X may very will insist that some explanation *for the resemblance* is called for, in addition to those explanations that we would *all* agree can be found for the dream, and for the state of affairs. Y, a "skeptic," may well regard the resemblance as derivative, secondary, not something that calls for a special explanation unique to it. It is for this reason that they are at loggerheads over the proper assessment of the situation. Each can say, echoing Rhine and Pratt, that a certain feature of the situation is not explainable in terms of the usual principles of explanation, just as my student and I agree about that. But X and the student feel that something is "left over" which needs to be explained, whereas Y and I do not agree that it is a *deficiency* that there is no explanation for what we regard as derivative features of the phenomena. It is for this reason, primarily, that Rhine and Pratt are wrong in going on to assert that this inexplicability must be due to something independent of those principles of explanation. It seems instead to be due to what has been selected from the situation as standing in need of (separate) explanation. Hence, it does not follow from the fact that putative cases of clairvoyant dreams cannot be wholly explained in terms of physical principles that they must involve a mode of perception that is independent of the senses.

Consider one other example from Rhine and Pratt. They continually speak of psi "capacities," even though they admit that "psi" is simply a "general term to designate the whole range of parapsychical phenomena" (p. 9). It then seems natural to them to say this:

> The occurrence of interaction between psychical and physical systems [in mind and body] implies to the logical mind a basic unity suggesting that the phenomena of parapsychology and physics are both of [*sic*] the same all-embracing universe. If so then a larger scope of reality is still to be disclosed than has been as yet revealed. Physics, then, is not unrelated to psi and its operation. We can say, rather, only that psi is not describable in terms of physical processes. (p. 11)

True, but vacuously so. Phenomena to be designated as "psi" have been picked out precisely on this basis. We do not have something independently identified which is then found to act other than physical laws would lead us to expect. The phenomena are isolated *because* they (or some feature of them) act otherwise than physical laws would lead us to expect. We still have two

very important possibilities open to us other than the one adopted by Rhine and Pratt, namely (1) that physical laws can be revised so as to account for these phenomena, and (2) that some features of the world have been arbitrarily, and unjustifiably, selected as *warranting* explanation.

I shall say nothing in this paper about the problem in the first possibility, but I do wish to emphasize the importance of the second possibility. We have seen that the disagreements concerning the appropriate evaluation of putatively paranormal phenomena stem partly from disagreements about what phenomena, and what aspects of phenomena, require a "non-derivative" explanation—that is, what phenomena require to be explained in their own right, and not just as consequences of other phenomena. I have emphasized this in opposition to the assumption that everyone agrees that something peculiar has happened, calling for special explanation, when someone has a putatively clairvoyant dream, or when a better-than-chance run is established in some laboratory card-calling tests. The problem with this assumption is that there are all kinds of "coincidences" that could be picked out as standing in need of explanation. There are presently as many students enrolled in one of my classes as there are days in the current month. Seventy is the mandatory retirement age in my institution, and 1970 was the year I began teaching there. And so on and so on.

Why do some cases seem so much more interesting than others? One possible response (implied in the quote from Rhine and Pratt in the middle of page 171) must, I think, be rejected. It may be said that we are interested in spontaneous phenomena that have been shown by laboratory tests—where we can specify exactly what chance, and extra-chance, results would look like—to occur with some regularity in the world. We know from card-calling experiments, it may be said, that clairvoyance is possible in certain laboratory situations. Hence all other clairvoyant cases are rendered more believable, or at least more noteworthy. This reasoning is backwards. The spontaneous cases came first, and the laboratory work was initiated in order to have a more orderly method of assessing the likelihood of such phenomena. Nor would it do, I think, to say that, however we may have come to be interested in clairvoyant phenomena, laboratory work has now shown that interest to be justified. I shall return to this point.

Why, then, are we struck by cases of seeming clairvoyance? My feeling is that we do not proceed on the basis of reasons in counting such cases as important. Instead, they strike us as important and then we, taking that importance for granted, attempt to explain the thing that struck us. I should like to devote more attention to the phenomenon of our *being struck* itself. It would seem that being correlated in time or in space cannot (*logically* cannot) be the sole criterion for our picking out of the welter of events in the world those whose relationships deserve to be investigated. If so, it must be largely a psychological matter that something strikes us as "more than coincidental."

But this seems to me to involve issues in psychology and the nature of operant conditioning that are beyond my competence. Moreover, it may be that such speculations would never go very far toward resolving the issue. For these reasons, I shall devote the remainder of this paper to an examination of some problems facing someone who wishes to provide reasons for believing that some set of events that others regard as only coincidentally associated are really related in some more interesting way.

Such an argument will be hard to make out by reference to the general nature of coincidence. For one thing, skeptics say things like this: "The number of events in which you participate for a month, or even a week, is so huge that the probability of noticing a startling correlation is quite high, especially if you keep a sharp outlook" (Gardner, 1972, p. 69). Again:

> the world in which we live is an extremely complex sample space, in which it is doubtful whether there are any "laws of chance" which apply to many of the single events occurring in it. Coincidences are certainly to be expected, and the sheer number may be felt to build up a case for a force or agent which is metaphysical, supernatural, or at least not part of the current corpus of science. But the mere accumulation of instances has less to do with probability than with the striking force of coincidence. . . . Science has not ignored some underlying order; [instead,] it has not yet devised ways of protecting us against spurious evidences of order. (Skinner, 1977, p. 11)

Some people regard it as unsurprising that the world contains surprising coincidences. Moreover, it is troublesome that (as I have earlier pointed out) the coincidences that can be noted are indefinitely numerous. It would seem that so long as we remain at the level of generalities, we will not be able to decide whether coincidences are evidence of some as yet hidden order, or "spurious evidences of order."

Parapsychologists themselves look to what they believe to have been established by laboratory research as a general solution to the separation of genuine coincidences from more significant happenings. Here is an example:

> Until the discovery of ESP, the easiest way to dispose of any puzzling experience now recognizable as involving it was to say it was only a coincidence. . . . Against this formula no one attempting to sort out presumptive instances of ESP from other kinds of experiences also coming from unconscious sources had a decisive argument until the actual existence of the ESP ability had been proven in the laboratory. . . . Now, knowing ESP does occur, one can claim for it with a much higher degree of assurance than before those experiences that fulfill the definition. (L. E. Rhine, 1967, p. 45)

There seem to me to be some problems with this kind of confidence. I will express them in terms of our continuing discussion of coincidence, but it will be noted that these are traditional, even venerable objections against the

reliance on laboratory results. Whether, as I hope, it will be helpful to cast them in this relatively novel light remains to be seen.

Someone who does not believe that there is any kind of underlying order of the sort parapsychologists seek will look at these experiments as evidence of some other misleading factor, just as I look at the fact that a woman *seems* to be sawed in two and then to be whole again as evidence that some other hidden feature(s) of the situation must exist. Nor is the skeptic obliged to turn to fraud as an explanation. There are the well-known possibilities of unconscious recording errors, faulty experimental design, and many other culprits. What the existence of these alternatives illustrates is that the assessment of such phenomena must be based on the assumption of some explanatory framework or other. Parapsychologists seem to see what they are doing in the laboratory as a *preliminary* to the provision of some kind of explanatory framework. But the way they interpret the results shows that they are assuming such a framework all along, and one that varies in a far too ad hoc way to be satisfactory. Rhine and Pratt, in discussing the effects of distance on ESP results, claim that there is no appreciable effect of distance in the Pearce–Pratt series. But they can say this only by explaining away what variation *does* occur on the basis of "psychological factors." This does not seem a warranted procedure, because we have no idea whether psychological factors should be expected to influence psi the way they do more usual perceptions. If psi violates so many other regularities, why hold this one constant? We simply do not have enough of a theory to enable us to discriminate distance variations from psychological factors. (See J. B. Rhine and Pratt, 1957, pp. 67–68; also Murphy, 1961, pp. 154–155.)

It would appear that those characteristics are assigned to psi that are necessary to explain deviations from expectations. When Basil Shackleton's results began to deviate in the experiments with Soal, it was found that his answers corresponded to future targets. He began to "hit" the target that was observed *after* the one that was being observed when he actually made the guess. Soal began to suspect precognition. When the speed of calling was doubled, Shackleton began to "hit" the target *two* places ahead of the current one. This seemed to Soal and Bateman to make good sense. But is this really a case, as Gardner Murphy claims, where the "evidence groups itself according to some inner logic of its own"? The procedure, he tells us, "is to let nature group data for us as she likes" (1961, p. 154). But surely a *choice* is involved when we see the Shackleton results as successful precognition and not (say) unsuccessful (simultaneous) telepathy, or even unsuccessful postcognition?

This kind of theoretical looseness can also be found in the evaluation of spontaneous phenomena. Louisa Rhine describes a case communicated to her laboratory by a woman in California, in which her very strong wish that

her husband (stationed in post–World War II Japan) would call her at 11:00 P.M. on a certain night was fulfilled by a call at 11:05 P.M. from the husband, who had just arrived in San Francisco. He, however, knew nothing of her need to speak with him about their son's health, since her letter on that topic, with its request that he call her when he did, had reached Japan only after he had left. He had, as it turned out, simply received his orders to return to the States at an unexpectedly early time, and was calling his wife on arrival. Louisa Rhine says:

> Precognition? . . . Possibly it could explain the setting of the date when her husband would telephone. But the rest—her husband's orders to return and the actual timing of his call—must be relegated to coincidence. Even if more might have been involved, it is better in studying ESP to decide conservatively. (1967, p. 44)

But why not suggest that psychokinesis was involved? We are told that the woman wished very much to speak with her husband. If relatively weak wishes about little plasticene cubes can deflect their courses of flight, why might not much stronger wishes alter the course of the dispensing of military orders? At the very least, we can easily imagine other parapsychologists taking this route, rather than agreeing to relegate such a striking coincidence to the scrap heap. The conservatism Rhine recommends is at best inconsistently applied.

An important part of our understanding of the world lies in knowing what *not* to try to explain. We neglect to explain far more than we explain, and this is a result of, as well as a crucial determinant of, the serviceability of the explanations we *do* give. To call something a coincidence is not necessarily to be mean and lunkheaded; it may point to the fact that the explanatory frameworks that are being accepted do not show this event to be something that warrants a separate, non-derivative explanation. To urge that someone treat some such event as not a coincidence after all is to encourage him or her to dislodge at least part of the explanatory framework by which sense is made of the world, and important features of it separated from unimportant features. It is not unreasonable to treat such urgings with the caution and conservatism that our very serviceable explanatory frameworks have partially justified. Demands for repeatability of experimental results have more to them than a desire to fit parapsychology into some pre-existing and questionable scientific mold. We need something to pry us out of our usually justifiable confidence in our ordinary understanding of the world. We *may* not feel any more regret about not being able to explain a putative laboratory demonstration of clairvoyance than we do about failure to account for a lucky run at cards. *Some* unexplained events and states of affairs are inevitable. Demand for repeatability is demand for one of the criteria that we ask a phenomenon to meet before it is considered other than a coincidence.

Suppose my phone rings just as I turn on my office lights in the morning. There is no one on the line. Struck by this, and by the simultaneity of the ringing with the lights' coming on, I turn the lights off and turn them on again. The phone rings again. I do this several times, and the concomitance is always repeated. At some point, I would no longer seriously entertain the suggestion that the conjunction of switching and ringing is coincidental. But now suppose that when I tried the switching the second time the phone had not rung. I continued to make tests, and to be struck by the first time's concomitance, but I could not get the phone to ring again. This would not *prove* that the switching did not cause the ringing in the first case, but it would make the contention less plausible. Moreover, it would not be rational procedure for me to fabricate some complicated explanation for why, even though the switching *did* cause the ringing the first time, it did not on any of the later trials. To insist that in certain special cases where we have no consistent repeatability, and also have no theoretical understanding of a possible connection, we must nevertheless be able to *discover* some theoretical underpinning shows a procedure remarkably divergent from our usual approach to unknown areas. To use ad hominem arguments against people who fail to agree to the strange procedure compounds the divergence. We do not expect the ringing of phones to be connected to the switching on of lights, and we demand something more than one occurrence of seeming connection before we surrender that expectation. We also do not expect seemingly haphazard guesses to conform to a random sequence of symbols with any accuracy, and it seems rational to insist on as much demonstration in this case as we would demand in the other. It is not the fact that events cannot be explained by science that will convince skeptics of the existence of hitherto unnoticed "underlying order." Something has to convince us that that failure of explanation is worthy of serious notice.

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Parapsychology: Science or Pseudoscience?

ANTONY FLEW

1

One thing has to be said with emphasis at the start. It is that the case of parapsychology is quite different from most of the others falling within the scope of the Committee for the Scientific Investigation of the Claims of the Paranormal.[1] It is quite different, that is to say, from the factitious, but richly profitable mysteries of the Bermuda Triangle and of the Chariots of the Gods, from astrological prediction, from the extraterrestrial identification of Unidentified Flying Objects, or from most of the other affairs dealt with so faithfully in that committee's useful and entertaining journal *The Sceptical Enquirer*.[2] The crucial difference from these other cases mentioned is that there we either know from the beginning that it is all bunkum, or else we can come to know this very soon after serious and honest investigation has begun.

Thus the moment someone concerned to discover what's what, rather than to produce a best-selling real life mystery, began to probe the Bermuda Triangle story it became apparent that there is no sufficient reason to believe that more ships and aircraft vanish without trace in that area than anywhere else with comparable traffic densities and comparable natural hazards. Again, there just is no good reason to believe that there have been any close encounters of the third kind, nor indeed of the first nor second either. The truth here is that the content of visions, dreams, and misperceptions is always in part a function of the wishes, beliefs, and expectations of the subject. So Chinese, under the old Emperors, used to dream dreams of dragons and Confucian officials; but not of Red Guards, chanting doubleplus good Chairman Mao-think. So too Bernadette Soubirois in her nineteenth century French village had a vision of the Blessed Virgin, as represented in pictures and images in her local church; but not of Shiva the Destroyer, as

represented in Indian temple sculptures. So, again and likewise, when contemporary North American readers of science fiction misperceive celestrial phenomena, what they believe that they have seen is neither gods nor a dragon but a spaceship. Such false identifications are, in one of the finest phrases of Karl Marx, "the illusion of the epoch."

Parapsychology, however, is a horse of quite another color. One of the properly uncelebrated silver jubilees of 1978 was that of the publication of my own first book, entitled in English English, with all the brash arrogance of youth, *A New Approach to Psychical Research*.[3] Yet it is just worth saying here that, after reviewing the literature as it then was, I concluded there that, although there was no repeatable experiment to demonstrate the reality of any of the putative psi-phenomena, and although the entire field was buried under ever-mounting piles of rubbish produced by charlatans and suckers; nevertheless one could not with a good academic conscience dismiss the case as closed. Too much seemingly sound work pointing to the genuineness of at least some of these phenomena had been done. Too many honest, toughminded, methodologically sophisticated and often formidably distinguished persons had been involved in this work. Not even the youngest and most wholehearted of Humians could recommend that we commit it all to the flames as "containing nothing but sophistry and illusion."[4] The research had to go on.

With, it must be confessed, precious little participation by the author of "that juvenile work," the research has indeed gone on.[5] In all probability its sum in the years between is as great or greater than the total for all the years before. Yet it is hard to point to any respect in which the general situation is better now than it was then. Certainly there is still no repeatable experiment to demonstrate the reality of any putative psi-phenomenon. Now as then the experts are inclined to construe the night on night regularity of the performance of any stage or screen psychic as proof that the performance is nothing but conjuring. Even worse or—according to taste—even better S. G. Soal's work on Gloria Stewart and Basil Shackleton has been progressively discredited. This won Soal a D.Sc. from the University of London, and was hailed by so tough a nut as C. D. Broad as involving, among other things, "The Experimental Establishment of Telepathic Precognition."[6] Nevertheless, not to put too fine a point on it, Soal, who was in his later years to present the crudely fraudulent Jones brothers as *The Mind Readers*,[7] seems to have been faking the scores.[8]

Having so far, in the present Section 1, labored in the main to distance parapsychology from some wholly disreputable exercises in deception and self-deception, I intend in the remaining three sections to consider three respects in which it appears to differ from all the established high-status sciences. First, its field has to be defined negatively. Second, there is no repeatable demonstration that it does in truth have its own peculiar and

genuine data to investigate. And, third, there is no even half-way plausible theory with which to account for the materials it is supposed to have to explain.[9]

2

In his Gifford lectures, Sir Charles Sherrington remarked that the names given to the vitamins were at first "non-committal in order that scientific ignorance should not be cloaked. Under fuller knowledge they are already being christened properly and chemically. Vitamin C is ascorbic acid . . ."[10] It is now usual for parapsychologists to begin by following this excellent example; although here, regretttably, there is no sign of progress towards legitimate re-christenings. "Parapsychology" is thus defined as "the study of the psi-phenomena"; "psi" being the name of the initial letter of the Greek word from which our "psychic" is derived. Psi-phenomena are divided into two fundamental categories: psi-gamma; and psi-kappa. "Gamma" and "Kappa" are again names for Greek letters; the initial letters, respectively, for the Greek words for knowledge and movement. The word "psi-gamma" covers both of what are elsewhere more tendentiously described as "clairvoyance" (clear seeing) and "telepathy" (distant feeling). The word "psi-kappa" substitutes for the equally tendentious "psychokinesis" (movement by the mind).

(i) We speak, or would speak, of psi-gamma when some subject comes up with information; and when that subject's acquisition of this information cannot be put down either to chance, or to peception, or to inference from materials ultimately obtained through sensory channels. These phenomena, or alleged phenomena, are then subdivided in two ways. One distinction is between clairvoyant and telepathic conditions. The idea is to distinguish two kinds of psi-gamma information: that already available to some person other than the subject, and presumably being somehow acquired from that other person; and that not available to any other person but immanent in the non-personal world, and presumably being somehow acquired directly from that non-personal world. The tradition, strongly challenged yet dominant still, takes a Platonic-Cartesian view of the nature of man for granted. So it describes the former as mind to mind, the latter as matter to mind.

The other distinction refers to temporal order. If the information produced in or by the subject is only going to become normally available in the future, then it is usual to speak of paranormal precognition or of precognitive psi-gamma. With appropriate alternations the same formula will give the meanings of "paranormal retrocognition" and "retrocognitive psi-gamma." When there is no such qualifying adjective we may take it that the psi-gamma is neither precognitive nor retrocognitive but simultaneous.

Once these several definitions are given and understood, it must become immediately obvious that it is inept—not to say perverse—to characterize

psi-gamma as a new form of either perception or knowledge.

(a) If the word "extra" in the expression "Extra-Sensory Perception (ESP)" is construed as meaning outside of—like the "extra" of "extra-marital sex"—then that expression becomes self-contradictory. It becomes equivalent to "extra-perceptual perception"; and hence, as Thomas Hobbes would have had us add, parallel to "incorporeal substance." If, on the other hand, "extra-sensory" is interpreted as referring to an hypothetical additional sense, then that hypothesis is at once falsified by two decisive deficiencies. First, there is no bodily organ or area masking or local anaesthetization of which suppresses psi-gamma. Second, there is no accompanying sixth mode of sensory experience as different from visual, tactual, gustatory, auditory, and olfactory as each of these is different from all the others. For good measure we may conclude the paragraph by mentioning a further deficiency. It seems that the subjects who come up with the information are unable at the time to recognize the deliverances of this supposed new sense, and to distinguish them from plain ordinary guesses or hunches or imaginings.

(b) Since psi-gamma information is defined as precisely not being acquired through the senses it really is, as has just been urged, perverse to insist upon thinking of psi-gamma in terms of a perceptual model. It is almost equally perverse to think of such information as constituting a kind of knowledge. For the definition stipulates that the subject must not be in a position to know, either on the basis of perception, or on the basis of inference from antecedently available material. If, but only if, subjects were at the time of coming up with the information able to pick out some of the items as coming from a fresh, special and reliably veridical source; then indeed we might quite properly begin to speak of belief in the truth of these items as knowledge, knowledge duly grounded in that source or faculty. But that ability is no part of the accepted definition of "psi-gamma." Nor would it be sensible to require it by adding a further clause. For it appears that such ability is rather seldom claimed, and never in fact found, among those responsible for what are, on the established weaker definition, ostensible cases of psi-gamma.[11]

Since the suggestion that we have here a fresh form of knowledge is, for the reasons given, wrong, I regret that no one took up my proposal to make the temporal distinctions by applying to the Greek noun "psi-gamma" the appropriate member of a trio of more familiar Latin letters: M (for minus, replacing retrocognitive); S (for simultaneous); or P (for plus, replacing precognitive).[12]

(ii) For present purposes the most important feature of the definitions presented in Subsection (i) is that they stipulate what psi-gamma is not, rather than what it is, or would be. In the opening words of one especially thoughtful address: "The field . . . must be unique in one respect at least: no other discipline, so far as I know, has its subject matter demarcated by

exclusively negative criteria. A phenomenon is, by definition, paranormal if and only if it contravenes some fundamental and well-founded assumption of science." [13]

(a) There is, I imagine, no disputing but that this must make it harder to establish that there really is psi-gamma: the difficulty of proving negatives is notorious and trite. But some other consequences are less obvious and more disputatious. Take first the points of Subsubsection 1 (i) (b), above, and especially the last two; that subjects are not able to pick out items as coming from a fresh, special, and reliably [11] veridical source; and that it neither is nor ought to be part of the meaning of "psi-gamma," that they should be so able.

From all this it surely follows, as indeed is the case, that psi-gamma can only be identified by subsequent checkups; and hence that it is not an independent source of knowledge. Thus, in the experimental work, the only way of telling whether or not we have any psi-gamma effect is by scoring up the subjects' guesses against the targets, and then calculating whether the proportion of hits to misses is too great to be dismissed as no better than what could have been expected "by the law of averages."

The case is substantially the same with what Broad would have us describe as, sporadic rather than spontaneous, psi-phenomena. There is again no way of identifying information coming telepathically or clairvoyantly save by comparing the hunches, dreams, visions, thoughts or what have you of the subject with whatever it is to which they may or may not correspond; and then estimating as best we can whether or not the degree of correspondence is greater than might reasonably be put down to chance, perception, or conscious or unconscious inference from materials ordinarily available to the subject.

The conclusions that psi-gamma as at present defined can only be identified by subsequent sensory checkups, and that it is therefore not an independent source of knowledge, carry an interesting corollary. This corollary seems to have been noticed only once or twice, and never discussed.[14] It is that, even supposing that we were able to construct a coherent concept of an incorporeal soul surviving the dissolution of its body, we could not consistently suggest that such souls might first learn of one another's existence, and then proceed to communicate, through psi-gamma.

For suppose first that there were such incorporeal Cartesian subjects of experience. And suppose further that there is from time to time a close correspondence between the mental contents of two of these beings, although such a fact could not, surely, be known by any normal means to anyone in either our world or the next. Now, how could either of these two souls have, indeed how could there be, any good reason for hypothesizing the existence of the other; or of any others? How could such beings have, indeed how could there be, any good reason for picking out some of their own mental contents as—so to speak—messages received; for taking these but not those to be, not

expressions of a spontaneous and undirected exercise of the imagination, but externally provoked communication input? Suppose these two challenging questions could be answered, still the third would be "the killing blow." For how could such beings identify any particular items as true or false, or even give sense to this distinction?

The upshot seems to be that the concept of psi-gamma is essentially parasitical upon everyday, this-worldly notions, that, where there could not be perception, there could not be "extra-sensory perception" either. It is assumed too often and too easily that psi-capacities not only can be, but have to be, the attributes of something immaterial and incorporeal; mainly for no better reason than that they would be non-physical in the quite different sense of being out of touch with the scope of today's physical theories. Yet the truth is that the very concepts of psi are just as much involved with the human body as are those of other human capacities and activities. In the gnomic words of Wittgenstein: "The human body is the best picture of the human soul." [15]

(b) A second important, but too rarely remarked, consequence of the fact that the definition of "psi-gamma" is negative, is that the concept itself, and not just the best available evidence that it does in fact have some application, is essentially statistical. Consider first a standard experiment in which a subject guesses through a well-shuffled pack of Zener cards–five suits of five identical cards–while an agent, suitably concealed from the subject, exposes to himself, and briefly contemplates, each card in turn. We are, that is, supposing telepathic as opposed to clairvoyance conditions. And suppose that, after this procedure has been many times repeated, it emerges that the subject has scored significantly better, or worse, than the expected chance average rate of one in five. Then on the face of it we have a case of psi-gamma.

But now notice that we have absolutely no way of picking out from the series any single hit, or any collection of particular hits, and identifying this, or these, as due to the subject's psi-capacity, rather than to chance. Or, rather, that is misleading. It is not that we as a matter of fact at this time cannot thus divide the singly paranormal from the singly normal. The crux is that no meaning has been given to this distinction: psi-gamma just is understood as a factor which manifests itself, if at all, only in the occurrence of significant deviations from mean chance expectation over a series of guesses–or over a series of whatever else it may be.

This is also one of the reasons why it is misleading to speak of a subject who puts up a score significantly better, or worse, than mean chance expectation as doing this *by* or *by means of* telepathy, clairvoyance, or other paranormal power. For while it remains possible, at least as far as the present consideration goes, for theorists to hypothesize some so far unrecognized kind of radiation through which information is conveyed to subjects; still "psi-

gamma" is at this time defined as precisely not the product of any means we can think of.[16] If the subject used the methods of the conjuror, or cheated by stealing a peek at the target cards, or had some hand in the determination of their values, then the results are on these grounds disqualified as not genuine psi-gamma.

Some have thought to dismiss the contention of this subsection on the ground that it does not apply to sporadic psi-gamma. But it does. Consider, for instance, the person who—"on the night when that great ship went down"—had a dream which both they and the parapsychologists are inclined to rate as telepathic. Their case will rest, not upon any particular correspondence between the dream images and the reality, but upon the total amount of that correspondence. A perfect fit of the whole would be the sum of fits at every particular point. Once again, there is no way of determining, and no sense in asking, which of these particular fits should be scored to chance and which to psi-gamma.

(iii) In this paper I am concentrating on psi-gamma, without asking systematically how much of what I say applies to psi-kappa. But I cannot leave the point about the essentially statistical character of the former without—not for the first time—drawing attention to a most remarkable fact. The fact is that all the evidence for the latter, and almost all the work on it, is similarly statistical. Yet the concept of psi-kappa is not. In the Glossary printed in every issue of the *Journal of Parapsychology* "psi-kappa (PK)" is defined as "the direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation." More popularly, it is the putative power to move something, or at least to impress a force upon it, by just willing, and without touching it or employing any electrical or mechanical device to bring this result about.

Now, it should be immediately obvious that there is no analogue here for those mere chance correspondences which investigators of psi-gamma through their statistical calculations labour to discount. There seems to be no a priori reason why psi-kappa should have to be detected and studied as the production, by a subject just "willing," of a significant surplus of sixes among the falls of dice mechanically rolled ten or more at a time; rather than as the production, by the same subject just "willing," of particular single movements in some highly sensitive and scrupulously shielded physical instrument. On the contrary: it would seem a priori far more likely that subjects would be able to direct their—shall we say?—willpower at a single stationary target than at (presumably at most one or two of the) several dice moving rapidly yet raggedly in midair. For would not such direction require a find-fix-and-strike mechanism comparable with what is needed in an anti-ballistic missile (ABM) defense system?

Of course, nature neither has to be nor is slave to our notions of the a priori probable or improbable. So it may be that in fact it is easier or only possible

to deploy the force—May the force be with you, investigators!—against either a confusion of dice spinning in midair or a jostling mob of paramecia. But the observation of the present subsection must still raise questions about experimenters who seem never in the first decade or so, and rarely later, either to have effected tests of the most obvious kind, or to have provided any rationale for their long-sustained refusal to do so.[17] Rhine, I believe, spoke truer than he either knew or would have cared to know when, in 1947, he insisted: "The most revealing fact about PK is its close tie up with ESP. . . ."[18] For the uncanny resemblances between, on the one hand, the methods and findings of the experimental investigation of psi-gamma and, on the other hand, those of psi-kappa, do in truth constitute strong, though much less than decisive, reason for concluding that what we have in both cases is evidence, not so much of some previously unrecognized personal power, but rather of a lot of fraud, self-deception or incompetence—and maybe of some real statistical oddities not significant of causal connections. There certainly is "Something Very Unsatisfactory" about what is supposed to be evidence of putative personal power, yet in which there seem to be no close concomitant variations between the effects alleged and any psychological variables in the supposed effectors.[19]

3

One of the most important similarities between the two main subareas of the field of parapsychological experimentation is that, typically, the work of one investigator cannot be repeated by another, not even when the second is able to use the same subjects as the first.[20] This fact is one of several which give purchase for the representation here of Hume's once notorious arguments about the difficulty, amounting usually to the impossibility, of establishing upon historical evidence that miracles have occurred.[21] These arguments were thus a few years ago redeployed in the present context by G. R. Price.[22] This Price, it has to be said, must not be confused with two others better known in this field: the late disreputable Harry Price, who surely faked some of the Borley Rectory phenomena which he was pretending to investigate; and the most excellent sometime Oxford professor, well known for an almost Kantian integrity.

Hume, it will be remembered, contended "that no testimony for any kind of miracle has ever amounted to a probability, much less to a proof; and that even supposing it amounted to a proof, it would be opposed by another proof; derived from the very nature of the fact, which it would endeavor to establish." Confronted by such a conflict of evidence, and—the interpreter must interject—remembering Hume's unfortunate ambition to develop a psychological mechanics, "we have nothing to do but substract the one from the other, and embrace an opinion, either on one side or the other, with that assurance which arises from the remainder." However, for reasons which are

not made altogether clear, "this substraction, with regard to all popular religions, amounts to an entire annihiliation; and therefore we may establish it as a maxim, that no human testimony can have such force as to prove a miracle, and make it a just foundation for any such system of religion."[23]

(i) A miracle for Hume would be much more than a fact "which ... partakes of the extraordinary and the marvellous." For, by the force of the term, "A miracle is a violation of the laws of nature . . . ." (A footnote adds a supplementary clause: "A miracle may be accurately defined, *a transgression of a law of nature by a particular volition of the Deity, or by the interposition of some invisible agent*.")[24] Waiving on this occasion the scholarly question whether Hume himself was in any position to provide an account of laws of nature strong enough to permit this contrast between a miracle and a fact which merely "partakes of the extraordinary and the marvellous," we need first to show that reports of psi-phenomena would lie within the range of Hume's argument.

(a) There is no doubt but that they would, or do. Certainly it is not easy to think of any particular named law of nature–such as Boyle's Law or Snell's Law or what have you–which would be, or is, as Hume would have it, "violated" by the occurrence of psi-gamma or psi-kappa. What that threatens is more fundamental. For the psi-phenomena are in effect defined in terms of the violation of certain "basic limiting principles;" principles which constitute a framework for all our thinking about and investigation of human affairs, and principles which are continually being verified by our discoveries. If, for instance, official secret information gets out from a government office, then the security people try to think of every possible channel of leakage; and what never appears on the check lists of such practical persons is psi-gamma. When similarly there has been an explosion in a power station or other industrial plant, then the investigators move in. At no stage will they entertain any suggestion that no one and nothing touched anything, that the explosion was triggered by some conscious or unconscious exercise of psi-kappa. Nor shall we expect them to turn up any reason for thinking that their, and our, framework assumptions were here mistaken.

It is some of these usually unformulated "basic limiting principles" which both psi-gamma and psi-kappa would, or do, violate, and which C. D. Broad formulated in his much reprinted *Philosophy* article on "The Relevance of Psychical Research to Philosophy."[25] Broad's formulations here are pervasively Cartesian. They thus provide for "the interposition of some invisible agent," if not for "a particular violation of the Deity." What, for instance, psi-kappa would violate is the principle that "It is impossible for an event in a person's mind to produce directly any change in the material world except certain changes in his own brain. . . . [I]t is these brain changes which are the immediate consequences of his volitions; and the willed movements of his fingers follow, if they do so, only as rather remote causal descendants."[26]

A Rylean, of course, would attribute any psi feats to the flesh and blood person rather than to his putative incorporeal mind or soul. But Broad, taking absolutely for granted a fundamentally Cartesian view of the nature of man, is instead so misguided as to conclude that it is the supposed establishment of the reality of the psi-phenomena, rather than this unnoticed and unargued preconception, which "has undermined that epiphenomenalist view of the human mind and all its activities, which all other known facts seem so strongly to support . . ."[27]

(b) In their first response to G. R. Price's paper "Science and the Supernatural" Paul Meehl and Michael Scriven wrote: "Price is in exactly the position of a man who might have insisted that Michelson and Morley were liars because the evidence for the physical theory of that time was stronger than that for the veracity of these experimenters."[28] It is important to appreciate why this is not so. Two of the reasons I shall consider here and now; the third is the subject of Section 4, below.

First, the Michelson-Morley experiment was not one in a long series including many impressively disillusioning instances of fraud and self-deception. Second, there was in that case no reason at the time—nor has any reason emerged since—for suspecting that the experiment would not be repeatable, and repeated; as well as confirmed indirectly by other experiments similarly repeatable, and repeated. It is these two weaknesses together which lay parapsychology wide open to the Humian challenge, each weakness reinforcing the other. The black record of fraud would not carry nearly so much weight against what might seem to be strong new cases of psi; if only we possessed some repeatable demonstration of the reality of such phenomena. We should not be in such desperate need of that repeatable demonstration; if only there had not been so much fraud and self-deception.

This is perhaps the moment to perform the nowadays mandatory genuflexion towards Thomas Kuhn's *The Structure of Scientific Revolutions*. Normal science—here to be construed as contrasting with pseudo-science rather than science in revolution—involves "research firmly based upon one or more past scientific achievements . . . that some particular community acknowledges for a time as supplying the foundation for its further practice."[29] Such an acknowledged achievement, if the acknowledgement and diploma title "science" are to be deserved, must surely embrace some measure of demonstrable repeatability. For—becoming now a brazen and reactionary non-Kuhnian—remember that the aim of science is, after discovering what sorts of things happen, to explain why: the qualification "sorts of" has to go in to cover the point that, unlike history, science is concerned with the type rather than the token. The formula for the repetitive production of a type is at the same time an initial, no doubt inadequate, explanation of the occurrence of any and every particular token of that general type, while

in these two aspects together the achievement of that formula constitutes a pledge of more and better yet to come.

So, until and unless the parapsychologists are able to set up a repeatable demonstration, they will at best be making preparations for the future development of a future science—with no guarantees that these aspirations ever will in fact be realized. One moral to draw from this point, and indeed from the whole paper in which it is made, is that, if affiliation to the AAAS is thought of as a recognition of actual achievement rather than of good intentions, then the Parapsychological Association is not yet qualified for admission, and ought now to be politely disaffiliated.

(c) It is sometimes suggested, either that repeatability does not matter, or else that there already is as much of it in parapsychology as there was in say the study of magnetism before electricians learnt how to construct artificial magents, or as there is now in abnormal psychology.[30] But these analogies break down. Certainly alleged star performers in psi-gamma or psi-kappa are, like natural lodestones or calculating boys, rare. But, when the latter are found, different investigators regularly repeat the same results. The same, unfortunately, is not true with psi.

Nor will it do to dismiss the demand for repeatability as arbitrary or unreasonable. For, if only it could be satisfied, then parapsychology would escape the Humian challenge. But, as it is, any piece of work claiming to show that psi-phenomena have occurred is in effect a miracle story. So, in order to form the best estimate we can of what actually happened, we have to resort to the methods of critical history. This means that we have to interpret and assess the available evidence in light of all we know, or think we know, about what is probable or improbable, possible or impossible. But now, as we saw earlier in the present subsection, psi-phenomena are implicitly defined in terms of the violation of some of our most fundamental and best evidenced notions of contingent impossibility. So, even before any Humian allowance is made for the special corruptions afflicting this particular field, it would seem that our historical verdict will have to be, at best, an appropriately Scottish, and damping: "Not proven."

(ii) Hume started with a general argument about the difficulty of establishing upon historical evidence the occurrence of a miracle. He then proceeded to contend that this difficulty is compounded when the miracle stories in question have "regard to . . . popular religions." So much so that he felt entitled to conclude "that no human testimony can have such force as to prove a miracle, and make it a just foundation for any such system of religion."

Whatever force Hume's worldly contentions here may have must bear equally against the miracle stories of parapsychology. For the Founding Fathers unanimously believed that to establish the reality of what we now call the psi-phenomena would be to refute philosophical materialism; thus

opening the way to an empirically grounded doctrine of personal survival, even personal immortality. Frederic Myers, for instance, in his 1900 Presidential Address to the original Society for Psychical Research (London) said in as many words, that their goal was to provide "the preamble of all religions," and to become able to proclaim: "thus we demonstrate that a spiritual world exists, a world of independent and abiding realities, not a mere 'epiphenomenon' or transitory effect of the material world."[31]

Again, Henry Sidgwick in his own second Presidential Address, speaking of the motives of the whole founding group, explained how "it apeared to us that there was an important body of evidence–tending *prima facie* to establish the independence of soul or spirit . . . evidence tending to throw light on the question of the action of mind either apart from the body or otherwise than through known bodily organs."[32] In his third Presidential Address he added: "There is not one of us who would not feel ten times more interest in proving the action of intelligence other than those of living men, than in proving communication of human minds in an abnormal way."[33]

In our own day J. B. Rhine's best-selling accounts of the research at Duke University present it all as proving some sort of Cartesian view of the nature of man, and refuting philosophical materialism. "The thread of continuity," he writes, "is the bold attempt to trace as much as we can see of the outer bounds of the human mind in the universe."[34] Descriptions of familiar flesh and blood creatures guessing cards, or "willing" dice to fall their way, are spiced with references to minds; their powers, frontiers, and manifestations; their unknown, delicate and subtle capacities; and the experimental findings are all construed as striking hammer blows for "spiritual values" in the global battle against "materialism." Always Rhine deplores "the traditional disinclination to bring science to the aid of our value system."[35]

It is, by the way, a noteworthy indication of the enormous power and fascination of the Cartesian picture that, as we have seen, even so acute and so unspiritually-minded a professor as Broad took it as obvious that to establish the reality of paranormal human powers is both to establish the reality of incorporeal thinking substances as the bearers of those powers, and to undermine the plausibility of the epiphenomenalist account of the relation between consciousness and the conscious organism. Yet what reason did Broad have for attributing these putative powers to such unidentified and unidentifiable metaphysical entities,[36] rather than to those familiar flesh and blood creatures who to the philosophically uncontaminated eye are the ostensible performers?

4

In Section 1, I distinguished, as the third peculiarity prejudicing the scientific pretensions of parapsychology, the fact that "there is no even half-way plausible theory with which to account for the materials it is sup-

posed to have to explain." This deficiency bears on the question of scientific status in two ways. For a theory which related the putative psi-phenomena to something else less contentious would tend both to probabilify their actual occurrence and to explain why they do thus indeed occur. Here we have the third reason why to refuse to accept the reality of such phenomena is not on all fours with dismissing the result of the Michelson-Morley experiment. For, even if no one then was ready immediately with an alternative theory, still in that case there was no good reason to fear that such a theory could not be produced. But, in the case of parapsychology now, our investigators have had a hundred years for theoretical cogitation, while there is also reason to believe that at least some of the phenomena alleged are so defined as to be necessarily impervious to causal explanation.

(i) The situation is confused by the fact that most investigators have been, and are, attached to a conceptual scheme whose actual explanatory power they tend vastly to exaggerate. For, as we have seen, most of them, taking the Cartesian concept of soul to be quite unproblematic, are ready to construe any proof of the reality of psi-phenomena as at the same time proof of the existence and activities of Cartesian souls. So, as they come to believe in this reality, they forthwith attribute all such performances to those putative agents. When they leap to this congenial conclusion, not only do they overlook the by now surely notorious difficulties of offering any serviceable description to enable these incorporeal somewhats to be identified, individuated and reidentified through time, they also fail to provide their proposed hypothetical entities with any characteristics warranting the expectation that these could, and naturally would, achieve what for mere creatures of flesh and blood must be simply impossible. If, as C. W. K. Mundle put it in his 1972 Presidential Address to the Society for Psychical Research (London), "materialism is to be rejected in favor of dualism on the ground that materialism cannot explain all kinds of ESP, it needs to be shown that, and how, all kinds of ESP can be explained in terms of immaterial minds." [37]

Where detail is vouchsafed sufficient to yield a piece of discussable and even testable theory, the result is almost if not quite always a fragment, a fragment which could at best serve only to explain one kind of psi-phenomenon—simultaneous psi-gamma under telepathic conditions. This applies, for instance, both to Whately Carington's proposals about the association of ideas [38] and to Ninian Marshall's physicalistic postulation of an assimilative force by which all physical things tend to make others more like themselves.[39] But the evidence for straight simultaneous psi-gamma under telepathic conditions now appears to be neither substantially stronger than, nor of a significantly different kind from, the evidence for "precognitive" psi-gamma, simultaneous psi-gamma under clairvoyance conditions, or psi-kappa. So it looks as if our choice is: either to think up a comprehensive

theory covering all kinds of psi-phenomena, and presumably a lot else besides; or else to go back in the end to the position which we could not in good academic conscience adopt at the beginning—that of committing the whole pseudo-subject to the flames, in high Humian style, "as containing nothing but sophistry and illusion."

(ii) Already in Subsection 2 (iii) I hinted at the great obstacle in the way of an explanation of psi-kappa: this is the problem of describing some believable "find-fix-and-strike" mechanism for directing the force "at (presumably at most one or two of the) several dice moving rapidly yet raggedly in mid-air"; and when those dice or other larger objects may not be within the sensory range of the "willer." We have now to notice in passing a similar massive obstacle standing in the way of any attempt to explain psi-gamma under clairvoyance conditions. Suppose that someone does spectacularly better than mean chance expectation in guessing the values of the cards in a well-shuffled pack, guessing these "down through," with no one touching that pack until the complete guess-run is later scored. What conceivable mechanism could that subject have employed—unconsciously, of course—to acquire the information needed to achieve such scores? (An appreciation of the force of this question has held many psychical researchers back from accepting clairvoyance even when they have no remaining doubts about telepathy.)

(iii) These are both formidable difficulties for the speculator. But the obstacle barring the way to any explanations which accept the genuineness of P psi-gamma (precognition) is an altogether different kind, and totally decisive. For if the various conditions usually specified as essential to a genuine case of P psi-gamma are in truth all satisfied simultaneously, then what is going on just is not susceptible to explanation in terms of causes or of causally interpretable natural laws. It is significant that when Broad offered a theory "to explain precognition" he added this warning: ". . . notice that, on this theory of 'precognition,' no event is ever 'precognized' in the strict and literal sense."[40]

The crux is that inexplicability is built into what is here rather oddly called "the strict and literal sense" of (paranormal) precognition. For consider, to start with we must have highly significant correlations between what someone says, or does, or experiences and what is later said, or done, or happens. But though necessary this is by no means sufficient. If the subject played any part in bringing about those later ongoings, then that would be enough to disqualify that subject's anticipations as a case of P psi-gamma. Suppose next that the correlation between these anticipations and their later fulfillments could be explained in terms of some common causal ancestry: maybe the guesser is an identical twin of the person choosing the targets; and they share genetically determined patterns of guessing and choosing dispositions. Here too the correlations would be disqualified: it is indeed mainly

in order to avoid disqualification by reference to a common causal ancestry that experimenters insist that the targets must be randomly selected.

But now, what possibility of causal explanation is left? If anticipations and fulfillments are causally connected, then either the anticipations must cause the fulfillments, or the anticipations and the fulfillments must both be partly or wholly caused by something else, or the fulfillments must cause the anticipations. The first two disjuncts are, as we have just seen, ruled out by the force of the (expression) term (paranormal) "precognition." The third is radically incoherent. Because causes necessarily and always bring about their effects, it must be irredeemably self-contradictory to suggest that the (later) fulfillments might cause the (earlier) anticipations. By the time the fulfillments are occurring the anticipations already have occurred. It would, therefore, be futile to labour either to bring about or to undo what is already unalterably past and done.[41]

I indicated in the first paragraph of this final Section 4 how a well-supported theory may probabilify the occurrence of whatever it predicts. So I trust that it will not look like a lapse into anti-empirical dogmatism to conclude with a maxim from Sir Arthur Eddington, a leading British physicist of the period between the wars: "It is also a good rule not to put overmuch confidence in the observational results until they are confirmed by theory."

Notes

1. This was set up a year or two ago on the initiative of Professor Paul Kurtz of SUNY at Buffalo, then editor of *The Humanist*, in hopes of doing something to stem the rising tide of popular credulity.

2. Formerly *The Zetetic*, now edited by Kendrick Frazier from 3025 Palo Alto Drive NE, Albuquerque, New Mexico 87111.

3. London: C. A. Watts, 1953. This book was long ago remaindered, and its publishers were later absorbed into the fresh-founded firm Pemberton Books. But two chapters, revised, are still current in philosophical anthologies: see, for one, Note 35.

4. Hume, *An Inquiry Concerning Human Understanding*, XII (iii), *ad. fin.*

5. See the Advertisement, referring to the *Treatise*, added by Hume to what in the event became the first posthumous edition of that *Inquiry*.

6. Compare this article under this title in *Philosophy* (London) Vol. XIX (1966), pp. 261–75. Broad was only perhaps the most distinguished of the many who accepted this work at its face value. That throng—or should I say that rout?—included the author of *A New Approach to Psychical Research*.

7. London: Faber and Faber, 1959.

8. See, most recently, Betty Markwick, "The Soal-Goldney Experiments with Basil Shackleton: New Evidence of Data Manipulation" in *Proceedings of the Society for Psychical Research* (London), Vol. LVI (1978), pp. 250–78. Compare D. J. West, "Checks on ESP Experimenters" in the *Journal* of the same society, Vol. XLXIX (1978), pp. 897–9, also further references given in these articles and in Edward Girden, "Parapsychology," Chapter 14 of E. C. Carterette and M. P. Friedman (Eds.)

Handbook of Perception, Vol. X (New York: Academic Press, 1970), pp. 385–412, especially pp. 396 ff. All this sheds a very bright light upon Soal's "calm, but perfectly devastating reply" to B. F. Skinner's contention that mechanical scoring devices should have been employed, to reduce the possibilities of cheating. (See G. R. Price: "Where Is the Definitive Experiment?", reprinted in J. Ludwig (Ed.), *Philosophy and Parapsychology* (Buffalo, N.Y.: Prometheus, 1978), pp. 197–8.

9. Had I been writing this paper now I should have needed to make some reference to the recent ganzfeld work, which does at least appear to offer more promise of full repeatability than anything else so far achieved.

10. *Man on His Nature* (Cambridge, CUP, 1946), p. 96.

11. See, for instance, C. J. Ducasse in J. Ludwig (Ed.), *loc. cit.*, p. 131.

12. Almost everyone embarking on a discussion of 'the philosophical implications of (paranormal) precognition' has in fact got off on the wrong foot by first thinking of P psi-gamma on the model of either cognition or perception or both. See, for instance, my article "Precognition" in Paul Edwards (Ed.) *The Encyclopaedia of Philosophy* (New York: MacMillan and Free Press, 1967) Vol. I. pp. 139–50; or more fully, my "Broad and Supernormal Precognition" in P. A. Schilpp (Ed.) *The Philosophy of C. D. Broad* (New York: Tudor, 1959), pp. 411–35.

13. John Beloff, reprinted in J. Ludwig (Ed.) *loc. at.*, p. 356: the first printing was in the Society's journal, Vol. XLII (1963), pp. 101–16.

14. See my "Is there a Case for Disembodied Survival?" in the *Journal of the American Society for Psychical Research*, Vol. LXVI (1972), pp. 129–44; perhaps more easily found in J. M. O. Wheatley and H. L. Edge (Eds.) *Philosophical Dimensions of Parapsychology* (Springfield, Illinois: C. C. Thomas, 1976) or—in a revised and retitled version—in my own *The Presumption of Atheism* (New York: Barnes and Noble, 1976).

15. *Philosophical Investigations*, tr. G. E. M. Anscombe (Oxford: Blackwell, 1953), p. 278.

16. So far as I know the first person to make much of this point that psi-gamma is essentially meaningless was Richard Robinson. See his contribution to the Symposium "Is Psychical Research Relevant to Philosophy?", originally published in the *Proceedings of the Aristotelian Society*, Supp. Vol. XXIV (1950), but reprinted in J. Ludwig (Ed.) *loc. cit.* The relevant passage is at pp. 80–3 in that reprinting.

17. Compare my "Something Very Unsatisfactory," in the *International Journal of Parapsychology*, Vol. VI (1964), pp. 101–5.

18. See E. Girden "A Review of Psychokinesis (PK)" in the *Psychological Bulletin* Vol. LIX (1962), at pp. 353 ff.

19. Compare Note 17 above. The only apparent exception to the generalization in the text is the sheep/goat work initiated by Dr. Gertrude Schmeidler: the "sheep," who believe in the reality of psi-phenomena, usually put up better scores than the "goats," who do not. But, even if the findings here were unequivocal, they would not provide the sort of dramatic concomitant variation reported in—and, apparently, reported only in—the since discredited work of Soal: there when the conditions were suddenly and secretly changed from telepathy to clairvoyance one subject's scores at once dropped to a chance level, recovering equally immediately when the original experimental conditions were restored.

20. Compare, again, Girden *loc. cit.*

21. I develop and defend an interpretation of these arguments in Chapter VIII of my *Hume's Philosophy of Belief* (New York: Humanities Press, 1961). So here I follow without defending that same interpretation.

22. This Humian challenge was first published in *Science* CXXII (1955) No. 3165, pp. 359–67; but it is now more easily found, along with one or two counter-blasts, in Ludwig *loc. cit.*

23. *An Inquiry Concerning Human Understanding*, X (ii); p. 127 in the standard Selby-Bigge edition. I suppose that "the principle here explained"—a principle which so remarkably ensures that "substraction" of the lesser quantity from the greater, neither leaves the greater undiminished, nor diminishes it by the amount "substracted," but instead always yields a zero remainder—is the sum of all the considerations, which Hume has been deploying in Part II of this Section X, for thinking that the evidence for the occurrence of miracles, which, if they occurred, would support "popular religions," is peculiarly rubbishy and corrupt. The upshot should be that it is all this superstitious evidence which is thus annihilated, and hence that there is here nothing to "substract" from the strong contrary evidence that whatever laws are in question do in fact obtain universally. But this is not what Hume actually wrote; which is, it has to be admitted, just muddled.

24. *Ibid.*, pp. 113, 114 and 115*n*: italics original.

25. *Philosophy* for 1949 (Vol. XXIV), pp. 291–309; reprinted in J. Ludwig *loc. cit.*, pp. 43–63.

26. *Ibid.*, p. 46: italics original. This principle is Broad's "(2) Limitations on the Action of Mind on Matter."

27. *Ibid.*, p. 63; and perhaps compare my *A Rational Animal* (Oxford: Clarendon, 1978).

28. J. Ludwig *loc. cit.* pp. 187–8.

29. (Chicago: University of Chicago Press, 1962), p. 10.

30. For the second of these see M. Scriven "The Frontiers of Psychology" in R. G. Colodny (Ed.) *Frontiers of Science and Philosophy* (Pittsburgh: University of Pittsburgh Press, 1962), reprinted in Wheatley and Edge (Eds.), *loc. cit.* pp. 46–75. The relevant paragraphs are in this reprint at pp. 64–5.

31. *Proceedings of the S. P. R.*, Vol. XV, p. 117.

32. *Ibid.*, Vol. V, pp. 272–3.

33. *Ibid.*, Vol. V, p. 401.

34. *The Reach of the Mind* (London: Faber, 1948), p. 50.

35. *Telepathy and the Human Personality* (London: Society for Psychical Research, 1950), p. 36. For critiques of such Cartesian misconstructions compare: first, my "Minds and Mystifications," in *The Listener* Vol. XLVI (1951), pp. 501–2, reprinted in P. A. French (Ed.) *Philosophers in Wonderland* (St. Paul, Minn.: Llewellyn, 1975) pp. 163–7; and, second, "Describing and Explaining," in the book mentioned in Note 3, revised and reprinted in J. Ludwig (Ed.) *loc. cit.*, pp. 207–27.

36. Compare Terence Penelhum "Survival and Disembodied Existence," a pot-pourri from his book under the same title, cooked up specially for inclusion in Wheatley and Edge *loc. cit.*, pp. 308–29.

37. "Strange Facts in Search of a Theory," in Wheatley and Edge (Eds.) *loc. cit.* pp. 76–97: the sentence quoted is at p. 88.

38. *Matter, Mind and Meaning* (London: Methuen, 1949), pp. 203 ff.

39. ESP and Memory: A Physical Theory," in the *British Journal for the Philosophy of Science* Vol. X (1960), pp. 265–86.

40. *Religion, Philosophy and Psychical Research* (London: Routledge and Kegan Paul, 1953), p. 80.

41. Compare the unusually hard-hitting symposium "Can an Effect Precede Its Cause?", in *Proceedings of the Aristotelian Society*, Supp. Vol. XXVIII (1954), pp. 27–62. By altogether ignoring the second contribution A. J. Ayer contrives to repeat in

his *The Problem of Knowledge* (Harmondsworth: Penguin, 1956), pp. 170–5, the main mistakes of the first.

Nowadays someone is likely to object that modern physics gives hospitability to the notion of backwards causation. Here I can and will do no more about this than quote yet again from Broad, this time pillaging his contribution to J. R. Smythies (Ed.) *Science and ESP* (London: Routlege and Kegan Paul, 1967), p. 195: "May I add that it would not be enough to cite eminent physicists who talk as if they believed this. What is nonsense if interpreted literally, is no less nonsense, if so interpreted, when talked by eminent physicists in their professional capacity. But when a way of talking, which is nonsensical if interpreted literally, is found to be useful by distinguished scientists in their own sphere, it is reasonable for the layman to assume that it is convenient shorthand for something which is intelligible but would be very complicated to state in accurate literal terms."

Philosophical Difficulties with Paranormal Knowledge Claims

JANE DURAN

Are precognition, telepathy, and clairvoyance possible? This is, to some extent at least, a philosophical question, and one that has been treated by philosophers. The field of paranormal research in general—whatever may be encompassed by that label—calls for philosophical examination precisely because some paranormal claims seem to clash with our twentieth-century presuppositions about reality.

In this paper I want to review and evaluate some of the philosophical charges that precognition, telepathy, and clairvoyance are not possible. In a latter section I shall concentrate in particular on the claim that any of the phenomena commonly labeled telepathy, clairvoyance, or precognition can be said to amount to knowledge.

Conceptual Difficulties with Psychic Phenomena

In "The Relevance of Psychical Research to Philosophy," the philosopher C. D. Broad [1] lists that he terms *basic limiting principles*, part of the intellectual framework we carry with us in the conduct of our ordinary affairs. Some of these limiting principles appear to conflict with the claims of parapsychology. If they do so conflict, and if we have good reason to stick with such limiting principles, we have good reason to view parapsychological claims with a suspicious eye.

Broad's *basic limiting principles* are these:

(1) *General principles of causation.* Stated crudely, the import of this set of principles is that any event that is said to cause another event (the second event being referred to as an "effect") must be related to the effect through some causal chain, and must precede the effect temporally.

(2) *Limitations on the action of mind on matter.* The only kind of direct change in the material world that can be produced by "mind" is one of changes in brain state. All other putative effects are indirect, not direct.

(3) *Dependence of mind on brain.* A necessary condition of any event in what we refer to as "the mind" is an event in the brain itself. Mental events of differing persons are the direct results of events in differing respective brains.

(4) *Limitations on ways of acquiring knowledge.* If a person makes a knowledge claim that is not spurious, whatever is known could only be known to the epistemic agent as the end product of a causal chain that at some point involved the sense organs of the knower.

It is easy to see that some of the types of events that fall under the rubric of the parapsychological would apparently violate one or more of these principles were they actually to occur. It can be argued that precognition, which may be defined as knowledge of an event before it has occurred, violates the causal principles articulated in (1), and violates the epistemic principles adumbrated in (4). Telepathy, or communication between parties with no physical interaction involved, has been deemed to violate (4). If vision must be causally related to what is seen, clairvoyance—or the ability to visualize events that are not physically present—would seem to violate both (1) and (4).[2]

All of this poses a problem for the possibility of parapsychological phenomena, however, only if principles (1) through (4) are as basic as Broad takes them to be and only if the apparent conflicts cited above are more than merely apparent. Let us take each of these questions in turn, beginning with an examination of Broad's principles themselves.

Broad makes the following claims with regard to his limiting principles.

> These principles do cover very satisfactorily an enormous range of well-established facts of the most varied kinds. We are quite naturally inclined to think that they must be all-embracing; we are correspondingly loath to accept any challenged fact which seems to conflict with them; and, if we are forced to accept it, we strive desperately to house it within the accepted framework. But just in proportion to the philosophic importance of the basic limiting principles is the philosophic importance of any well-established exception to them. The speculative philosopher who is honest and competent will want to widen his synopsis so as to include these facts; and he will want to revise his fundamental concepts and basic limiting principles in such a way as to include the old and new facts in a single coherent system.[3]

In considering the principles as formulated, we must first note that those who are "naturally inclined to think that they must be all-embracing" are in general only the educated minority of western societies. Broad no doubt takes this for granted, but it is worth calling attention to. Many or most of the inhabitants of more technologically primitive societies, and many less educated inhabitants of western nations, are not naturally inclined to think along the lines of these principles, nor would they be inclined to think that such principles were all-embracing. Principle (3), for instance, has received the support it has only within the past few hundred years. Historically, the

tendency has been to separate the mental or spiritual from matter, rather than to assert its dependence on it. Then, too, principle (4) conflicts with the long-standing traditions of many, if not all, of the world's major religions. To the extent that a religion has embraced any of the doctrines of its mystical subsects, it has usually held that some knowledge not obtained through the senses is possible.

But even if we grant that to certain educated people the limiting principles seem to have enormous appeal, we must also examine how it is that phenomena such as telepathy, clairvoyance, and precognition violate one or more of these principles, if in fact they do. Let us take each principle in turn.

a. Principle (1) is highly important, and the standard line has it that precognition and clairvoyance would violate this principle. Precognition would apparently demand that an earlier event (a vision or dream of a plane crash, for example) be caused by a later event (the plane crash itself). True clairvoyance would seem to require causal action without any intermediate sequence of events. But our post-Humean model of causality is such that it is simply what we mean by "A causes B" that A precedes B temporally and that B is directly or indirectly the result of A through some sequence of events.

C. J. Ducasse, C. W. K. Mundle, and Antony Flew (in the preceding selection) have each emphasized this point. Ducasse insists on the importance of temporal sequence to causality:

> But I say that the only conclusion that is open to any English speaking, or rather English understanding reader, when he is told that an effect may well be supposed to precede its cause, is either that the asertion is startlingly false, or else that in spite of appearances, it is not really expressed in English, but in some other tongue in which the words "cause" and "effect" also occur, but surely with very different meanings indeed than in English.[4]

Mundle notes that:

> It appears self-contradictory to say that an event which has not yet happened, not yet "come into existence", could be either an "object of non-inferential knowledge" or "a cause-factor influencing what happened earlier." The latter phrase is certainly self-contradictory as the word "cause" is normally used, for it is part of the meaning of "cause" that a cause must precede its effects.[5]

But, it might be objected, is it not possible that usage of words is the crux of the matter here? Could it be argued that the notion of a cause's preceding its effect is strictly conventional? One might find this sort of argument appealing, and might thus propose a revised version of causality. A cause, one might say, is something related to its effect directly or indirectly, preceding it or occurring subsequently, such that the effect is somehow contained in the cause and the usual criteria of one following on the other—"constant conjunction"—apply. On this view, it might not be unnecessarily procrustean to

say that my dream of a plane crash was caused by the plane crash (which occurred at a later time) simply because the sequence of events in the dream seemed to be contained in the sequence of events in the plane crash itself. Isomorphism, or congruence, would then be one of our criteria for isolating a cause; if the cause seemed to take place later than the "resultant" event, we might label as the cause whatever later event proved to be most congruent to the earlier occurrence.

It appears to be something like this line that currently appeals to a number of those who believe, however vaguely, that their precognitive dream or visual experience is causally related to the concomitant later event. The problem with all this is that it still ignores other crucial aspects of what it means for a thing to function as a cause. If my dream of a plane crash is, on my view, the result of a given plane crash, and if the dream may be said to have taken place at time t_1 and the crash itself at time t_2, what I am using as my criterion for causality here is the putative isomorphism, and nothing more. Not only is it unclear how a causal chain could work backward in time; in this instance we have nothing resembling constant conjunction because very few dreams turn out to be isomorphic to later events. (It is worth noting that the number is so few that it is commonly held to be a matter of note when there is some congruency.) In other words, temporal precedence, the establishment of a traceable chain, and contiguity over a series of similar repetitions are at the heart of our notion of cause. *Mere* word usage cannot be cited as the crucial factor here. Thus the charge that precognition and clairvoyance violate principle (1) does seem to be well-founded, and not answerable by a proposed change of the meaning of "cause." But what of other supposed inconsistencies?

b. Principles (2) and (3) are linked, and might be held to constitute an argument against the possibility of telepathy.

Some version or variant of principle (3) is today referred to in philosophical circles as an "identity theory"—crudely put, the identity claimed is between minds and brains.[6] Some identity theorists would simply claim that thoughts or beliefs *are* brain states. But the minimum requirement for an identity theory would seem to be that what one *does* as a result of what one is thinking is the result of changes that are first made at the neurological level of brain cells, and that what one *thinks* is somehow the result of activity in these cells or other neurological areas in the first place. But if I were to obtain some piece of information telepathically, I have obviously gotten it in a way not involving the senses (presumably also violating principle (4)), and since the senses themselves are part and parcel of the neurological system, it would appear that principle (3) is violated. Quite simply, telepathy would demand that I know something without the involvement of the brain.

But some philosophers, John W. Godbey Jr. among them, have argued that some sort of identity theory is in fact compatible with this type of

parapsychological claim. Godbey's counterargument to the standard objection is disarmingly simple:

> The fact that people can learn about distant facts other than by present sense perception, memory and inference does not show that their minds are not brains. Such data would be consistent with, and in fact would seem to imply, merely that these people can acquire information in ways other than normal. Parapsychological data only demonstrate capacities of the mind which exceed any known capacities of the brain. [7]

The key sentence is the last one. A claim of telepathic experience does not show that the mind is not dependent on the brain; it may merely show that there is more to the principle that minds reduce to brains—or more to brains—than we are now aware of. The general line in Godbey's response is that what is "paranormal" about many of the cases that might appear, on their face, to violate principle (3) is the manner in which the knowledge was acquired. But the fact that the *manner* was "paranormal" does not itself show that the mind is not a brain, nor that the mind is something more than a brain.

Godbey's argument seems well taken. It may be the confusion or ignorance regarding the manner in which the knowledge was acquired that has led some to conclude illegitimately that paranormal manner implies paranormal mind substance. In any case, although objections to some paranormal claims on the grounds that they violate principle (1) appear acceptable, according to our earlier analysis, this is not the case for all objections based on the point that principle (3) or a conjunction of principles (2) and (3) is violated. It is by no means clear that a telepathic claim violates principles (2) and (3), or either one separately.

c. Let us finally consider the fourth of Broad's basic limiting principles. We have acknowledged at an earlier point that objections to both precognitive claims and claims of clairvoyance stem partially from the fear that principle (4) is violated. It now looks as if this principle would be relevant to the claims of telepathy as well. Principle (4) says somewhat long-windedly that knowledge may be had only if it is acquired through the senses. Interestingly enough, philosophers argued for centuries—until a comparatively short time ago—about the relative importance of sensory knowledge in comparison to knowledge obtained through "other" sources; so this principle, too, is one that is readily acceptable only to a certain modern cast of mind. Nevertheless we need to inquire as to whether any sort of claim of the three types already mentioned actually would violate principle (4).

Now it seems to be part of what is meant by a claim that precognition has occurred that the senses are only peripherally involved in the act of precognition, if at all. That is, we might concede from the outset that there is a

certain type of knowing before the fact that involves the senses—rapid and unusually acute accumulation of evidence, astoundingly acute induction, and so on—but that is not paranormal in the sense that my knowledge claim about a plane crash is if such a claim is based solely on a dream. The objection regarding the senses is not that a vision makes no use of any sense whatsoever, but simply that it arises in a way deviant from any way that might admit of normal explanation.

We know, for example, that certain intoxicating substances, including alcohol if ingested in sufficiently large quantities, can give rise to visions, yet the ingestion of these substances themselves accounts for the stimulation of the senses and seldom, it seems, is involved with any paranormal claim to knowledge. In the more typical case dubbed "precognitive" the agent is doing nothing that might be thought to be causally related to the vision—it arises spontaneously, as it were. The precognitive dream, although perhaps explicable on one level simply qua dream (unlike a vision) is frequently distinguished, so we are told, by its lifelikeness and clarity.

In these cases, then, whatever the causal chain produced by the senses, it does not seem to have its origin in anything that might explain the content of the vision or dream. Hence the claim that principle (4) is violated. A similar line of argument might be developed for the claims of telepathy or clairvoyance.

Now principle (4), if fully presented, relies on something like the following conception of sensory stimulation. In the case of my vision, I see veridically only when light rays reflected from some object within my line of sight strike my eye, stimulate the retina, and so on. No visual phenomenon not produced in this fashion could correctly be labeled "seeing" in the special case of "seeing" an afterimage or miniature pinkish elephants. Analogous statements could be constructed for the senses of hearing, smell, taste, and touch.

But do claims about precognitive visions really violate principle (4) in this regard? Not, I would claim, if the term "see" which often appears in such claims is taken to indicate only an isomorphism between the content of the vision and the later event "foreseen." It is widely reported in the research on cognition that many putative instances of precognition are so labeled only *after* the external event has taken place; Ducasse reports that "in a large percentage of them, no feeling of conviction distinguished the true ones [visions, auditory hallucinations, etc.] from the false."[8] Two points, then, can be made here. A precognitive (telepathic, clairvoyant) claim may not violate principle (4) because it may be claimed that the vision, auditory experience, or whatever is only some *analog* of a genuine case of seeing, hearing, and so on. But the use of the terms "saw," "heard," and so on in these reports (if in fact they are used) seems to stem not so much from the fact that the agent actually thinks he saw anything in the ordinary sense as from the fact that he

had a visual experience that gave rise to "knowledge" in a way analogous to the way in which visual perception gives rise to knowledge in the veridical case.

In any case, it is clear from the above that even if we may back off from claiming that principle (4) is violated in these instances of paranormal claims, we seem to be left with the conviction that principle (1) is violated. In the case of precognition, the reasons for thinking that principle (1) is violated have already been stated. In the cases of telepathy and clairvoyance, it would be the inability to specify the causal chain that would give rise to the sense of violation of principle (1). In these cases, the "cause" may very well precede the "effect." But the only causal chains to which we are accustomed to refer in such cases—causal sequences involving the senses—are absent in the cases referred to as paranormal.

The upshot of our examination of the limiting principles is that it seems almost certain that at least principle (1) is violated in every purported case of paranormal phenomena of the type under scrutiny here. Whether or not other principles may be thought to have been violated, the absence of a specifiable and recognizably causal chain seems to constitute a difficult, if not insurmountable, objection to our giving a coherent account of what it means to make such a claim. As long, at least, as our ordinary notions of causality remain intact, there seem to be strong philosophical reasons for concluding that telepathy, clairvoyance, and precognition are not possible.

Our Notion of Knowledge

Another set of difficulties concerns parapsychological phenomena and our notion of knowledge. Clairvoyance, telepathy, and precognition are commonly thought of as means of *knowledge*—extraordinary means, perhaps, but means of *knowledge* nevertheless. If we were to grant that each of these is possible, we still might question whether they can be regarded as means of knowledge.

The traditional account of knowledge consists of three elements. A person can be said to know that *p* if and only if: (i) *p* is true; (ii) *p* is believed; and, (iii) adequate evidence for *p* is had. This account has come under heavy attack in recent years.[9] Nevertheless, even modified versions of accounts of knowledge are built around the core of this version, justified true belief, or JTB. More recent versions usually attempt to incorporate a fourth condition of some sort.

Claims to knowledge based on precognitive, telepathic, or clairvoyant phenomena might be said to fail even the three-pronged versions of the necessary and sufficient conditions for knowledge. Leaving aside the first condition, conditions (ii) and (iii) cannot be met in the usual straightforward fashion where the claims to knowledge arise from situations involving the paranormal.

We mentioned at an earlier point that even such information as has been alleged to have been received through, for example, an instance of precognition does not seem to present itself to the recipient as immediately believable in the same way that information obtained in a more mundane fashion usually does. As Ducasse points out:

> True guesses or visions would have title to the name of perception and knowledge only if, in their case, the guesser's or dreamer's experience contained some feature—whether sensory or extrasensory—that were more or less a *reliable sign* that the guess or dream is true. But a study by Louisa E. Rhine of more than 3000 spontaneous "precognitive" and "contemporaneous cognitive" experiences, published in the June 1954 issue of the *Journal of Parapsychology*, shows that, in a large percentage of them, no feeling of conviction distinguished the true ones from the false.[10]

Some people may uniformly believe all "information" they receive by means of precognition and the like, some may be uniformly skeptical of it all, but in either case evidential matters seem to play no role in dictating belief for some items of "information" and skepticism for others. One might guess that certain receivers of information imparted in a paranormal manner choose to believe all of it, at least initially, since they are unable to distinguish true from false, or on some other ground. But the criterion of belief has traditionally been thought to be related to the criterion of evidence in some way; it is not merely that *p* is *believed*, but that one has *reasons for believing p*. Indeed, recent epistemology is full of examples of knowledge claims that rely on belief without rational basis and hence fail—for example, claims made on the basis of crystal-ball gazing.[11] So the kind of belief that one might have in the case of precognitive knowledge, for example, fairly clearly fails the test of fulfilling condition (ii) in the traditional three-pronged version of conditions for knowledge.

Condition (iii) is, if anything, even more problematic when viewed in this light. The paranormal experiences do not seem to be distinguishable on the basis of feelings of conviction as they occur. But for such an experience it might well be argued that "feelings of conviction" are all we have to go on. The ordinary standards of evidence clearly do not obtain in precognitive situations—we cannot appeal to the area that is itself under suspicion as being the evidential support for its claim. If one were to find a case in which a certain agent had met with *uniform* success in paranormal experiences, then that might conceivably constitute some sort of evidence (although even this is debatable), but no such situations are forthcoming.

But do conditions (i) through (iii) constitute an adequate account of knowledge? If not, it might be thought that parapsychology's failure to satisfy (i) through (iii) is no particular disqualification. And a justified-true-belief account has been challenged, using examples such as the following proposed by Edmund Gettier:

> Let us suppose that Smith has strong evidence for the following proposition:
> (f) Jones owns a Ford.
> Smith's evidence might be that Jones has at all times in the past within Smith's memory owned a car, and always a Ford, and that Jones has just offered Smith a ride while driving a Ford. Let us imagine, now, that Smith has another friend, Brown, of whose whereabouts he is totally ignorant. Smith selects three place-names quite at random, and constructs the following three propositions:
> (g) Either Jones owns a Ford, or Brown is in Boston;
> (h) Either Jones owns a Ford, or Brown is in Barcelona;
> (i) Either Jones owns a Ford, or Brown is in Brest-Litovsk.
> Each of these propositions is entailed by (f). Imagine that Smith realizes the entailment of each of these propositions he has constructed by (f), and proceeds to accept (g), (h), and (i) on the basis of (f). Smith has correctly inferred (g), (h), and (i) from a proposition for which he has strong evidence. Smith is therefore completely justified in believing each of these three propositions. Smith, of course, has no idea where Brown is.
>
> But imagine now that two further conditions hold. First, Jones does *not* own a Ford, but is at present driving a rented car. And secondly, by the sheerest coincidence, and entirely unknown to Smith, the place mentioned in proposition (h) happens really to be the place where Brown is. If these two conditions hold then Smith does *not* know that (h) is true, even though (i) (h) is true, (ii) Smith does believe that (h) is true, and (iii) Smith is justified in believing that (h) is true.[12]

If this is an adequate counterexample to "justified true belief" accounts, it appears that they are not entirely adequate. But does this vindicate parapsychological claims to knowledge? I think not. Notice how this sort of example differs from the type of problem posed by putative knowledge obtained through paranormal means. In the case of a Gettier-type example, both conditions (i) and (ii) hold—*p* is true, and is believed to be true by the agent. In the case of knowledge claims based on parapsychological phenomena, condition (i) must be ignored, since any attempt to deal with it will beg the question. But condition (ii) holds because the knower takes it that he had also fulfilled condition (iii)—he takes it that he has adequate evidence for his belief. Generally speaking, condition (iii) cannot be fulfilled under a paranormal knowledge claim, and if it should happen that condition (ii) is fulfilled, it will not be for reasons that relate to the typical condition (iii) reasons.

Thus we see that a counterargument to the effect that many apparent claims to knowledge fail the standard three criteria and that such failure constitutes no argument against paranormal claims does not go through because paranormal claims do not fail in the same way as the other cases—their failure is much more significant. Even if we were to accept clairvoyance, telepathy, and precognition as possible—despite the objections of the first section—we would still have reason to deny that they constitute means of knowledge.

Conclusion

In this brief critique of certain sorts of parapsychological claims, we have followed two major lines of argument. The first was that, even if it could be shown that some or most of Broad's basic limiting principles were not violated by the claims of precognition, telepathy, or clairvoyance, it seems clear that the first principle—the set of causal conditions—would be violated. This in itself constitutes a major objection to the claim that these types of paranormal phenomena are possible as sources of knowledge.

Second, we have acknowledged that it might be argued that certain claims to knowledge based on paranormal phenomena are somehow analogous or similar to other claims that might fail the ordinary criteria for knowledge. But we have shown that although it is true that both groups might be said to fail the traditional versions of JTB, paranormal claims do so in a significantly different way from, for example, Gettier-type problems. This would also seem to constitute a major objection to allowing such claims to count as knowledge claims.

Clearly, more work needs to be done on how principles (2), (3), and (4) might be violated before any definitive statements can be made with regard to the relations between paranormal claims and those principles. But the large conclusion of our thinking has been that, even if principles (2), (3), and (4) are not violated, the apparent violation of principle (1) combined with the violation of the relevant two portions of the JTB model should count heavily, if not decisively, against paranormal experience as actual knowledge.

Notes

1. C. D. Broad, "The Relevance of Psychical Research to Philosophy," in *Philosophy and Parapsychology*, ed. Jan Ludwig (New York: Prometheus Books, 1978), pp. 43–64; principles cited on pp. 45–49.

2. I exclude from discussion other phenomena that are frequently referred to in an exhaustive categorization of the paranormal, such as psychokinesis, survival after death, and so on, for the sake of manageability. It should be obvious that they merit similar treatment.

3. Broad, pp. 43–44.

4. C. J. Ducasse, *Causation and the Types of Necessity* (New York: Dover, 1969), p. 42.

5. C. W. K. Mundle, "Does the Concept of Precognition Make Sense?" *International Journal of Parapsychology* 6 (1964): 179–198, 182. Reprinted in Ludwig, *Philosophy and Parapsychology*, pp. 327–341.

6. Daniel C. Dennett, *Brainstorms* (White River, Vermont: Bradford Books, 1978), p. xii.

7. John W. Godbey, Jr., in *Analysis* (1975): 23.

8. C. J. Ducasse, "The Philosophical Importance of Psychic Phenomena," in Ludwig, p. 131.

9. Edmund Gettier, "Is Justified True Belief Knowledge?" *Analysis* 23 (1963): 121–123.

10. Ducasse, "Psychic Phenomena."

11. Keith Lehrer, "How Reasons Give Us Knowledge, or the Case of the Gypsy Lawyer," *Journal of Philosophy* 68 (1971): 311–313.

12. Gettier, pp. 122–123.

Precognition and the Paradoxes of Causality

BOB BRIER AND MAITHILI SCHMIDT-RAGHAVAN

The logical impossibility of a future event causing an event in the past is a well-known and widely accepted thesis. We propose to challenge the soundness of this dominant belief in the light of the results of an experiment on precognition conducted at the Institute for Parapsychology (formerly the Duke Parapsychology Laboratory) in Durham, North Carolina.[1] In what follows we shall argue that an explanation of the phenomenon of precognition involves a revision of the concept of cause. Once a surprising aspect of that concept is admitted, then precognition will be more easily assimilated into our world view. We shall first give an account of the experiment and its results. An atemporal notion of cause will then be defended with a view to accommodating precognition. Finally, we shall turn to particle physics and show the relevance of our conclusion regarding backward causation to that field.

There had always been talk around the lab of possible bizarre effects in ESP experiments. In the experiment we shall describe, we tested one of these possibilities, the effect of the checker (the person who scores a test) in precognition experiments. We had always wondered what the target was in a precognition experiment. Did the subject "see" the final record sheet? Did he "see" the cards in experiments where cards were used? Did he "see" the person who was scoring his record sheet, as well as the correct answers? If the subject perceived not only the answers, but the entire surroundings, then the surroundings could affect the scoring. If he did not like the person checking his guesses he might score negatively, and if he liked the person he might score positively.

In our experiment we tested groups of students by the standard precognition technique using random numbers. After the subjects had made their guesses, an entry point into the random number tables was found and the numbers were translated by a code into the five ESP symbols. Finally, a

checker entered the correct answers on the record sheet and tabulated the hits. Each subject did four runs of twenty-five guesses. In our experiment, the "experimenter" checked two of the runs and an "assistant" checked the other two. The subject never knew who would check which runs because this was randomly determined after the guesses were made and after the entry point into the table of random numbers was obtained.

The point is that, in this type of experiment, once the entry point is obtained, the scores are completely determined and it would seem that nothing done after can influence the scores. But in our experiment this was *not* the case. It mattered who scored the runs! There was a significant difference in scoring levels between the runs scored by the "experimenter" and those scored by the "assistant." The experiment was repeated a second time and similar results were obtained.

The phenomenon of precognition (as distinct from other forms of ESP such as clairvoyance or psychokinesis) appears to contradict certain widely accepted "a priori truths." For example, in this experiment it would seem that once an event (the hits being determined by the entry point into the table of random numbers) is fixed, nothing later (the selection of the checker) can affect it because the event is in the past. This self-evident truth seems to be contradicted by our results.

Backward Causation

We believe that it is possible for causes to come after their effects and that this backward causation may help explain how precognition works. Consider a standard precognition test in which a subject predicts which of five ESP symbols (star, waves, circle, square, or plus) will be selected the following day by a random procedure. If the subject repeatedly scores significantly above chance, we may be justified in believing that there is a causal connection between the guesses on one day and the actual order of the symbols on the following day. Which causes which? It is possible that the guess causes the selection of the symbols in some mysertious way. It is also possible that by backward causation the selection of the symbol causes the subject to have guessed that particular symbol. We could check to see if the process of selection is in fact random. (We could do this by checking the symbols generated for patterns or indications of non-randomness.) If it *is* random, a possible conclusion is that it was unaffected by the guesses, and it is the guesses that are caused by the symbols selected. This is not airtight, but it is the kind of evidence that might help to decide which of the two options to select. We might also consider spontaneous cases of precognition.

If a psychic precognizes a plane crash which in fact occurs on the following day, we again might wish to say that there is a causal connection between the plane's crashing and the precognitive experience—but which causes which? Here it seems more likely that the plane crashing caused the precognitive

experience rather than the other way around. Even if we knew of the precognition, we would still look for the effective cause of the crash. We would search the wreckage for signs of metal fatigue, an improperly hatched door, and so on. Thus we might locate the cause of the crash; but we must still locate the cause of precognition. It would seem to be the crash. So we would have some basis for choosing backward causation as our description of this pair of related events, as opposed to the equally mysterious forward causation. But this choice might be spurious.

Here it might be argued that backward causation is logically impossible. We are aware that most philosophers[2,3,4] who have considered the question of the logical possibility of backward causation have concluded that it is not logically possible, that in every instance where backward causation is assumed some contradiction can be derived. Elsewhere[5] the argument has been made that most philosophers who attempt to derive a contradiction from backward causation make some error. Indeed, they usually derive their contradictions by assuming that if backward causation were possible, then one could change the past. If something I do now can have its effect yesterday, or last year, why can we not change the past? Perhaps we are great fans of Napoleon and wish that he had won at Waterloo. Can we change history? The answer is no, we cannot *change* the past, but we can *affect* it. To make this clear, we shall borrow an example made up by Richard Gale.

Imagine that you are at a party on Saturday night and someone who works with your good friend Smith mentions that yesterday, Friday, he saw someone place a bomb in Smith's desk drawer. The bomb was set to go off in one hour. He also tells you that urgent business called him out of the office and he forgot to warn Smith. As a matter of fact, he has forgotton about the incident until now and does not know what happened to Smith. Is there anything you can do to save your friend? Seemingly not. Either he was blown up by the bomb, in which case he is dead, or he or someone else may have discovered the bomb and safely removed it (or the bomb did not go off, etc.), and he is now alive. In any case, there is nothing you can do about it. *If*, however, backward causation is possible, there may be something you *can* do to save your friend. While at the party you might call out, "Smith, this is me, warning you from the future. There is a bomb in your desk drawer. Get rid of it!"

So far there is nothing impossible about the situation we have described; it is merely somewhat bizarre. But now let us imagine that you meet Smith on the street the day after the party. He tells you of his strange experience on Friday. He was sitting at his desk and he had a vision of you at a party saying, "Smith, this is me, warning you from the future. There is a bomb in your desk drawer. Get rid of it!" He then found the bomb and safely disposed of it. What do we say of such a situation? We would say it is an example of backward causation. Something you did on Saturday (the warning called out

at the party) caused something to have hapened on the previous day (Smith's precognitive experience which in turn caused him to have been saved). Note, we do not say that you *changed* the past. It is not that Smith was blown up and then your warning changed this. Such a situation does involve a contradiction of the form $p \cdot \sim p$: Smith was blown up and Smith was not blown up. Had Smith been blown up, then you still could have tried to save him, but it simply would not have worked. What is the case is that if Smith was saved, it could have been because of something you did.

Once it is realized that backward causation involves merely affecting the past, and not changing it, then the contradictions dissolve. If this conclusion is correct, then it may have some relevance to particle physics where a rather similar controversy still continues.

Atemporal Causality

Our defense of an atemporal view of causality will consist in arguing for the soundness of the following theses:

1. Causal attribution imposes no requirement that the cause precede the effect; in other words, the condition of the temporal priority of cause need not be met in order to warrant the conferring of causality on a verified relationship between facts.
2. If no contradiction can be drawn by entertaining the idea of backward causation then it is a logical possibility.

A definition of causation might be approached by listing the conditions that many philosophers of science consider warrant the conferring of causality on a verified relationship between facts. Within science these causal guarantees are confined to the *necessity* and *sufficiency* of an association. Briefly, the criterion of "necessity" usually refers to the warrant for causality given to condition or event *c* if effect *e* never occurs without it. "Sufficiency" refers to one kind of invariance or "uniformity" criterion: whenever *c* obtains then *e* obtains, under $\text{conditions}_{1,2,3\,\ldots}$. The relation between cause and effect is thus a relation of *existential dependence*, the cause of an event being the condition or conditions "without which the event would not have occurred, or whose non-existence or non-occurrence would have made some difference to it."[6]

And here we find that the scientific view of causality differs from the popular view. In the latter that which is the cause is apparently antecedent in time to the effect and the causal relationship is held to be the production of an effect by some power residing in a temporally prior substance. But when, as in the scientific treatment of causality, we treat the cause of an event as the condition or the set of conditions that determine the event we find that these conditions are often contemporary with the effect. The cause of a disease, for example, is not antecedent to the course of the disease, if by antecedent we

mean that it produced the disease and then ceased to determine it. The presence of certain microorganisms that may be spoken of as a determining condition (i.e., cause) of the disease is contemporaneous with the disease itself. The scientific analysis of determining conditions does not make the causal relationship one of temporal sequence. There is no reason why we should not call an event a determining condition of another event even if it did not actually antedate the event.

This brings us to an essential point of distinction between the consideration of causal relations as efficient causes and the consideration of them as sufficient reasons. In the latter case we are concerned with establishing conditions under which causal status is conferred on *c* or conditions under which it is withheld from *c*; whereas in the former case our inquiry is into the conditions under which *c becomes e*. It is this that marks the distinction between the popular and the scientific view of causality. It also marks the distinction between the direct and the indirect method of difference in Mill's theory. The direct method of difference was formulated by Mill in these words:

> If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring only in the former; the circumstance in which alone the two instances differ is the effect, or the cause, or an indispensable part of the cause of the phenomenon.[7]

The use of this method depends on finding a positive and a negative instance agreeing in every circumstance but one. It is almost impossible to observe two instances agreeing in every circumstance except one, unless it is a case of experimentation. In the experiment a factor is introduced into the situation and a certain change ensues; when the same factor is left out, there is no change. This method, which is essentially a method of experiment, gives to "cause" its popular imprint of "force" or intervention. Mill's canon for the "joint method of agreement and difference" (also called the "indirect method of difference" by Mill) is as follows:

> If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance, the circumstance in which alone the two sets of instances differ is the effect, or the cause, or an indispensable part of the cause of the phenomenon.[8]

In this method we examine a number of positive instances to see if they agree in the presence of one common circumstance; we also examine a number of negative instances to see if these agree in the absence of that same circumstance. This method, as Mill puts it, "consists in a double employment of the Method of Agreement, each proof being independent of the other and

corroborating it."[9] In the joint method we have only observation of the joint presence and joint absence of two properties, but no indication of the temporal priority of either to the other. The rate of their joint occurrence (or non-occurrence) is the essential piece of information. For so long as the rate is sufficiently high to validate judgments of cause–effect connections, the requirement of temporal priority of the cause can safely be ignored in the attribution of causal relationship to an observed connection between events. The joint method thereby allows us to incorporate the possibility of backward causation within the framework of scientific use. If you decry our scruples in preferring the scientific view of causation to the popular or "interventionist" view, we need only point out that the *model of causation* is open and that as with any explanation the choice of model is in the service of present purposes.

The causal relation is not reducible by definition to the temporal one. However, the language is also characterized by criteria for the application of the causal predicate. In some causal explanations temporal sequence is mentioned among the criteria. The notion of a criterion is still a vague one in philosophy, but this much is clear about it, that a statement of a criterion is not aptly described as empirical, analytic, or necessary. Since it is not analytic or necessary it is both possible for a property to be present when a (selected) criterion for its predicate is not satisfied and conversely possible for the criterion to be satisfied in the absence of the property. Thus it is possible for x to be the cause of y, even though x does not precede y in time, but rather succeeds y in time.

Indeed it is doubtful if the cause in its entirety is ever prior to the effect. The analysis of "cause" as that which is temporally prior to the effect derives much of its strength from the fact that the cause has been considered as separable in time from the effect. And this notion of the temporal discreteness of cause and effect (on which the Humean analysis is based) leads to the absurd conclusion that if the set of conditions we call "cause" had ceased to be before the "effect" obtained then these conditions could not be held to be determining–and if they are not determining in any way then they cannot be said to "produce" the effect. Hence the criterion of the temporal priority of cause, far from being a necessary condition for the attribution of causality, militates against the popular view of causation. And any attempt to have this condition met must end in a fraud.

The demand for the employment of the temporal-priority criterion in causal attributions is based on a misunderstanding; one deriving from the implicit acknowledgment of the dispersal-of-order argument. This argument makes use of a specific kind of phenomenon such as entropic processes. There is the well-known example of a stone being thrown into a placid pool of water, and circular waves spreading out from the center being observed. Although the reverse process–concentric waves contracting toward a cen-

ter—is a logical possibility, it is never observed. If it were, it would be difficult to explain. One is used to explaining a number of coherent separated items (the waves) by referring to a single event (the dropping of the stone) causing them. If concentric waves were seen as converging at a center, it would seem that this would call for a third event that causes these waves as an explanation. Thus there seems to be a direction of influence, from a central event to coherent separated events. Thus it might be said that explanation runs to events that involve dispersal of order from events that do not. The foregoing is a sketch of a general argument showing why there is a direction of explanation with respect to the dispersal of order and suggesting why one takes the arrow of time as going in the traditional direction. It must now be shown how this relates to the direction of causation.

It is generally accepted that a cause explains its effect in a way an effect does not explain its cause. This is so even when the effect can be used to retrodict the cause. Given this, it is not unreasonable to hope that by considering how the explanatory powers of causes and effects differ, insight might be gained into the nature of causal priority. It must first be noted that when a question is asked by saying "Why?" the answer generally begins with "Because." That is, when one is asking the why of things one is asking for the cause; it is the cause that explains why the effect is. Clearly the effect does not explain why its cause is. All effect can do, if it is a sufficient condition for its cause, is guarantee that the cause occurred. This is not an explanation.

It is this unidirectional and irreversible nature of causal relationships that has been mistaken for temporal priority. We observe and call "causal" the ordering: "Ample rain: large wheat crop." We also allow an inference in reverse: "Small crop, therefore there must have been inadequate rain." But although the first sequence is "causal" we do not accord causality to the second. The asymmetry criterion is thus more important than the sequential. And the asymmetry criterion imposes no requirement that the cause precede the effect. It moves us away from thinking about temporal sequence and toward thinking about the "direction of influence."

Consider the case of a heavy metal ball resting on a soft cushion. Here is a case in which a cause and its effect are simultaneous, and temporal priority will not help to decide which is the cause and which is the effect. It would almost certainly be argued that the downward force of the ball causes the depression in the cushion, as opposed to arguing that when the ball is placed on the cushion, a depression appears and causes the ball to fall. The reason the first alternative is chosen is that taking the downward force exerted by the ball as the cause explains the depression. If the depression were taken as the cause, there would be need of an explanation as to how the depression came about. This example suggests that the kind of explanatory power possessed by a cause cannot be attributed to an effect. The causal relationship has been shown to be asymmetrical but atemporal.

The two models of causal explanations (which we have called the "popular" and the "scientific") are profoundly different, but our analysis has shown that they have the same logical texture: they are both adaptations of the principle of *sufficient reason.* Inasmuch as the backward causal explanation is an explication of this principle, it can be accommodated with confidence within the framework of causal explanations. Each kind of causal explanation is sought and achieved on a special level of inquiry, with its own data and its distinctive perspective; although in each case the mind works on the same logical principle, the principle of *sufficient reason.* As backward causal explanation does not militate against this principle, it cannot be ruled out a priori as a causal explanation.

This brings us to the second thesis, namely, the logical possibility of backward causation. The term "logically possible" is sometimes used in the sense of "logically conceivable" (i.e., it is self-consistent or implies no contradiction) and sometimes in the narrow sense of "imaginable" (i.e., it implies no absurdity). Take, for example, the expression "pitch higher than the highest pitch audible to the human ear." The response of dogs to whistles that produce air vibrations of such high frequency that the corresponding pitch is inaudible to the human ear proves that the statement, which is empirically confirmable, describes a literally unimaginable state of affairs. Although unimaginable it is logically possible in the sense that no contradiction is formally deducible from it. But that p is possible in the sense of being self-consistent does not entail that p is imaginable, as we have demonstrated. It is not enough to point out that backward causation is logically possible in the highly attenuated sense of "possible," namely, that it is self-consistent. For if we wish to make a case for backward causation we need to show that no absurdity is implied in imaging that "x causes y but x succeeds y in time." Our examples of precognitive experiences point clearly to the fact that "backward causal explanation" is not an inexplicable concept, and indicate that there are instances of the explicandum. "Causality" then can be explicated without reference to temporal priority.

Backward Causation and Particle Physics

In a now famous paper [10] Feinberg argued for the possibility of faster-than-light particles. These "tachyons" have spacelike four-momentum and consequently have velocities greater than c. Feinberg demonstrated that such particles do not violate the special theory of relativity, and he presented a quantum field theory for noninteracting tachyons. Arons and Sudarshan [11] rejected Feinberg's schema of quantization but still argued that such particles are possibilities and presented their own reinterpretation. Although tachyons have been empirically searched for, [12] physicists have questioned the logical possibility of there being such particles.[13,14]

The grounds on which the possibility of tachyons is questioned involve what have been called the *paradoxes of causality*. There are various versions of the paradoxes, but essentially they all involve the description of a particle going backward in time and the claim that this entails the paradox that one could communicate with one's past, change the past, describe the same event in contradictory ways, and so on. This has led physicists such as Arons and Sudarshan to reinterpret negative-energy particles going backward in time as positive-energy particles traveling forward in time (but in the opposite direction). It is important to note that when discussing the paradoxes of causality none of the authors ever derives a statement having the form $p \cdot \sim p$. They merely feel there is something peculiar involved and call it a paradox. If what we have suggested above is correct, then there is no reason why there could not be particles going backward in time, and therefore physicists should search for such entities. In support of the position that such a thing is possible, we might mention the work of physicists such as Csonka [15] who have constructed theoretical schemata in which particles with arbitrary quantum numbers interact with normal particles and there is full causality; that is, there is both retarded and advanced causality.

Notes

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Second Report on a Case of Experimenter Fraud

J. B. RHINE

It has been about fifteen months since Dr. Walter J. Levy, Jr.,[1] (W.J.L.) was discovered to be falsifying his test results in a PK experiment with rats at the Institute for Parapsychology, of which he was then Director. He was caught by three fellow staff members, James W. David, Jim Kennedy, and Jerry Levin, all of them members of his own research team. As soon as they considered the evidence conclusive, the three men who discovered the deception reported it to me; and after a short but decisive interview with W.J.L., I received his resignation.

Because W.J.L. had published many research papers and his work was widely known here and abroad, it seemed of first importance to warn readers against reliance on any of his research reports. The known evidence against him at the time of the exposé (June 12, 1974) concerned only one of his several lines of research, that in which rats were tested for PK, or psychokinetic ability. The experiment involved the implantation of an electrode in the pleasure center of the animal's brain to provide a means by which random electrical stimulation might make possible the arousal of the animal's desire to increase gratification by influencing (via PK) the rate of stimulation. W.J.L. himself would admit to no other falsification of results, but it seemed advisable to regard as "not acceptable" all reports of his research and all those in which he had played any part (Rhine, 1974b).

In my first report on the W.J.L. affair, I said that there should be a further report on later developments of the case. It is in compliance with that commitment that I now review what has happend thus far. First, a statement will be made about what is known of the extent of the dishonesty involved. Second, an evaluative word will be offered as to the consequences to the field.

I. The Known Extent of the W.J.L. Falsification

It was a natural question in the minds of all concerned as to how far the cheating exposed in the one experiment with rats extended throughout the rest of W.J.L's work and what, if anything, was left unaffected. Consideration was given at once to the possibility of making such a discriminative evaluation. It was reasonable to think that, with all the precautions that had been developed—especially since the elimination of the risk of fraud had long been on our minds—there might be some experiments by W.J.L. in which the conditions ruled out all possibility of dishonesty. It was soon realized, however, that although the precautionary test conditions had been intended to be the best that had been developed in psi research, there was always in actual practice some possibility still open to W.J.L. in his position as Director of the Institute and as a person who thoroughly involved himself in all that was going on. In fact, no one could say that the double-blind conditions, even with a design involving two or more experimenters in each experiment, had been so thoroughly observed that they had completely eliminated all possibility of dishonesty.

It was a unique situation. W.J.L. had been an extremely enterprising and industrious worker. As the chief administrator of the laboratory and an active leader in almost every aspect of its activities, he well knew all the limits and loopholes in a system he was masterminding seven days of the week—often sleeping in the attic while overseeing the 24-hour automatic testing of the animals. In view of these circumstances, there remained none of his work of which it could be said that he himself could not *possibly* have had any opportunity to manipulate the results that had been published. They all had to be put on the same level of uncertainty and unacceptability.

I think it can readily be seen that in the concern over an adequate reappraisal of his research there was nothing to do but to write it all off; and this I have done, at first only warning against any reliance on the W.J.L. reports for the time being. But on further reflection it became increasingly clear that the whole of his work was irretrievably lost, even without knowledge of the exact extent of the deception. In any case, such knowledge is not completely obtainable.

However, among individual members of the staff, especially those whose vigilance led to the discovery of the fraud in the first place, there remained unsatisfied questions. These led to some developments indicating that W.J.L's deception was much more extensive than had first been discovered. In one of these instances, first encountered by Jerry Levin, some indications turned up as to how W.J.L. could have manufactured results in the gerbil experiments on precognition. He could have done it by shorting one or the other of two wires that would have allowed him to manipulate the random target sequence as he watched the recording of the animal's behavior. It was W.J.L's uncalled-for presence at the apparatus with the wires in hand that

raised the first question for Levin and led to the exposé. Levin later on observed by accident the presence of fine scratches such as the wires would make if shorted to the aluminum panel.

Another such discovery was made by Jim Kennedy in an examination of the records of W.J.L's automated maze tests of clairvoyance with human subjects, the results of which were stored by the computer. This experiment, as devised by W.J.L., had first been conducted with paper-and-pen methods but later two series had been automated with the computer. Kennedy found that W.J.L's published results were not at all those recorded for the same experiments on the computer. (The original data were *not significant.*) Apparently a completely different set of results had been invented.

Still another discovery was made by Kennedy who had earlier done some PK testing under W.J.L's supervision using chicken embryos as "subjects." The aim had been to test for possible PK influence exerted on randomly operated heat lamps by the eggs (located as they were in a cool chamber). It had been noticed that there tended to be long strings of hits and an increased trial rate when the scoring was significant. The easiest way to manufacture high scoring in the egg work would produce such effects as these. When the strings were deleted from the data, the scoring rate was at the chance level. Similar stringing was produced as an artifact of W.J.L's admitted manipulation of the rat implantation work. Also, the records of egg research which W.J.L. conducted before Kennedy started working with him showed the presence of astronomically significant strings (of about 100 hits where the probability of a hit was 1/2), yet W.J.L. never mentioned them.

With all the grounds we have for withholding acceptance of W.J.L's work, we cannot—and certainly need not—draw a conclusion about the actual extent of the deception in the whole of his five years of reported research. But, as I have already indicated, no salvageable series or section of sound and significant results has thus far been isolated in which W.J.L. was not around somewhere within reach of an unprotected link in the chain of reliable controls. As will be seen further on, the safest and best type of evidence of psi emerges in the form of incidental hidden "signs of psi" (Rhine 1974a) which the experimenter could not himself have anticipated at the time. However, none of these exceptional fraud-proof signs has so far been found in W.J.L's work.

II. Consequences to Parapsychology of the W.J.L. Affair

The long-term effects of the W.J.L. case on parapsychology will probably depend mainly on the unity of response among colleagues in the field. It should therefore help in the long run in this unification if the fraud problem in general is kept in the focus of attention in the laboratories and the journals. This is not likely, I think, to be overdone and to create a morbid distraction to normal research. It will probably not arouse that much interest.

However, the fraud issue, even in its most generalized form, is but one of several major problems in keeping psi testing on a high level of security. The multiple aspect of research reliability was outlined at the beginning of an article on "Security versus Deception" (Rhine, 1974a). At the moment, however, the primary topic is that of fraud, and the specific issue is that of experimenter dishonesty. Yet, even now, it is still not just the deception of the W.J.L. type that we need to consider here. The very timing of this affair at the FRNM ties it up with the broader problem of possible experimenter dishonesty beyond this individual case and further complicates the search for adequate solutions. In fact, we can best regard the W.J.L. case as one that pointedly draws attention to a larger insecurity situation which, as I have already indicated (Rhine, 1975), was emerging well before the W.J.L. exposé took place.

Whatever the consequences of this fact are to be and whatever we are to do about them will likely be better understood if we take something of the earlier background into account. When we do so, it is much more understandable that we find ourselves in our present state of special concern over fraud in parapsychology. It is well known that Henry Sidgwick, the first President of the S.P.R., warned that when critics found no other points open to attack in psychical research, they would charge the investigators with fraud. That this would be the final recourse was, of course, logical. I can recall that in the 1930's experimenter fraud was something we had to force ourselves to take seriously. It was only after we had arrived at a relatively conclusive stage of the ESP tests that McDougall advised me to introduce the two-experimenter test design to meet the anticipated charge of fraud. The experimenter deception which this precaution was designed to exclude was not nearly so immediately urgent as had been the exclusion of sensory cues, the proper mathematical evaluations, the safeguarding of records, and the interpretations of the results. Even the critics did not begin to hammer effectively on the experimenter-fraud question until George Price's article appeared in *Science* (1955), and Price himself argued in effect that since no other criticism was crucial, the conclusion had to be fraud. So this was the predicted last resort of the skeptic.

I am reviewing this familiar situation as background for a closely related point that needs attention here: it is that the very same development that finally closed in on the critics' options, cornered the experimenter himself. At earlier stages of method, the experimenter could alternatively obtain spurious results under test conditions that allowed several uncontrolled factors, but one by one these gaps in method had been closed.

Those who understand science can see that this has been a progressive development in parapsychology in which not only is the unyielding critic driven by elimination into a last-ditch alternative of fraud, but also the experimenter who is unsuccessful in obtaining evidence of psi in his tests is

left with the single choice of accepting his failure and trying again or of falsifying his results.

I must interrupt at this point to comment that a few colleagues insist we are *making it too hard* for the experimenter by this gradual improvement of our methods so that at the end he has no alternative to the final evidence we are seeking. They hint that we are thus helping to make him cheat by our very measurement of success. Several spokesmen of this viewpoint are proposing that we lower the standards and abandon our reliance on the measurements of significance. They forget that in present psi testing this requirement of significance is the only way we *can* measure success. Might we not just as well say, "Let us freely allow sensory leakage"; or "Let us use no safeguards against recording errors." Surely we need not be frightened off our course of methodological advance by the fraud issue any more than we have been at other stages of the development of psi methodology. The way to help the less resolute experimenter is not to weaken the safeguards that make conclusions possible, but to strengthen the experimental program against error in every way.

My special point in drawing attention to the fact that dishonesty was the *only* option W.J.L. had was that, with the psi test methods what they are today, this alternative of deception was more easily and conclusively identified as such when the real evidence he had expected failed him. It was not hidden in a loose methodology that would have left the matter inconclusive and confusing. Bad as the situation was, therefore, we can still appreciate the advance in methods in parapsychology that facilitated the expose; we have therefore reached the crossroads which Price, Hansel, and others have long since recognized—the point where, from the view of extreme skepticism, "it had to be fraud." Very well; if that is where we are, it is to our advantage that now we have a clearly drawn contest, and I think we are better prepared to face it than ever.

But again, it is not just the critics' last stand; it is not only the experimenters' either. It involves the whole field quite as much. In the past the trouble has been that no more attention was paid to the question of experimenter fraud than the current urgency of a given case demanded. The principal lesson of the W.J.L. incident is to remind us of the lukewarm concern that has been felt generally throughout our field over the question of experimenter dishonesty. Now we must be ready, more than ever before, to give the attention this issue requires. It is mainly now a matter of how much we can accomplish toward that end while the urgency is still widely and keenly felt.

It is true, much should be accomplished in time by the kind of increasing safeguards that I have been discussing in recent years, and especially as others join the endeavor. Recognizedly, these past advances in methods, even while they do make fraud the only alternative to successful psi demonstration

in a well-designed experiment, will also make it equally more difficult for an experimenter to take that option and falsify his results. Better still, these advances in safeguarding methods should make the temptation to cheat less appealing and still more unrewarding as psi-testing designs improve. But I am coming to question, as I ponder the rections to the W.J.L. incident, whether we can afford to wait for this rate of advance. I find it easier to ask: "Why *should* we wait at all in this 'last-ditch' stage of indecisiveness? Is it really necessary?"

I have heard it stated at times in the last year that the suspicion of experimenter fraud could be expected to take an indefinitely long time to vanish completely from psi research. Today I do not think it has to be that way at all if we really take the problem seriously and all come to know the literature of the field adequately. In fact, I even think it should be possible to bring parapsychology through this final challenge to its security, not only with success but with reasonable dispatch. In making this proposal, however, I am assuming we will still go resolutely on with a vigorous development of the methods we already know, giving full attention to the safeguards we now have at our disposal for reducing the risk of experimenter deception. But it is time for a more conclusive step.

A Roundup of Fraud-proof Evidence is Essential

Since circumstances have made a mountain of apprehension over the fraud issue, an equally mountainous formation is called for to represent all the many types of fraud-proof evidence the field of parapsychology has to offer, most of which are little known as such even within the field. It is fair to say, too, that much more may yet be discovered that is still hidden from us all. This will call for a determined search; first, among the stores of existing records available for reexamination; and second, in new types of researches which lend themselves well to the purpose, as (judging by the past) many may be expected to do. It will suffice now, however, merely to indicate some of the principal kinds of evidence already known, to which the term "fraud-proof evidence of psi" can be applied. The category itself and its qualifications and limits can be refined later. The following few types will, I think, serve the purpose of introducing the wealth of this kind of material available without delay.

First, I would propose an examination of all available reports by scientific authors who for some reason wished not to publish their work, but who conducted acceptable experiments for their own satisfaction. As an example, I will cite the case of a report by an eminent scientist who was at the time the head of an internationally known institution. Many years ago he showed me his paper but felt he had to withhold it from publication in order to avoid embarrassment to his colleagues. He was, however, persuaded to publish the report of the experiment if all marks of identification of the author were

removed; and this was done. The results were significant and were even repeated later.

This type of incident has other variations. Sometimes the fear of losing his own status has kept an author from publishing, although he was willing to circulate a report privately. Or again, the author may earlier have become known as a critic of psi research and was reluctant to admit that he had later conducted an experiment and that significant results had been obtained. Still other varieties of this kind of situation, one in which it would not be reasonable to suspect the fraud motive, can be assembled. In many cases the actual names could not safely be used. The point these situations have in common is that it would not make sense even to suggest that there might have been any will or intention to fabricate the data. To be sure, such researchers have to be of good quality in other respects as well.

Second, I would draw attention to another type of incident that has occurred from time to time in the editorial offices of the *Journal of Parapsychology* or in the correspondence of the Institute for Parapsychology. In a typical case, a report of a psi experiment is received for publication from an author who is rather new to parapsychology. The results may or may not be reported as significant. In the course of editorial consideration of the paper it becomes evident that the results lend themselves to a type of analysis which is unknown to the author but which may be independent of the analysis he has already made. Or the author's attention may be drawn to a finding reported by another experimenter which was then found by the editorial staff to be confirmed by the data of this newly submitted report. When such an application is clearly new to the author and is based upon an earlier finding by someone else, it can not only be given full acceptance, but it is all the better for having been brought to the author's attention by an independent analyst. The author can, of course, then make an independent check on the analysis from his own records, i.e., to confirm the editor's findings. The fraud-proof value of these cases covers a considerable range of security, often well beyond that of experimenter deception; some are of the ideal type in which one effect is found to be a completely independent replication of an experiment made under a very safe set of conditions, as, for example, when independent analyses are made on duplicate records.

Third, even better than the preceding types, although somewhat more rare, are examples in which already published work may be reexamined long years after publication for a significant distribution of hit patterns in the data, free of any conceivable motivation on the part of either subjects or experimenters. Take, for example, the familiar telepathy tests by G.H. Estabrooks at Harvard in 1925 (Estabrooks, 1961). It will be recalled that he obtained positive results in card guessing in three out of four series of tests. In the fourth, in which the conditions were changed to a greater distance between subject and target, the group of subjects scored well *below* mean chance expectation.

Moreover, in all four series there was a marked top-bottom decline of scoring rate in the 20-trial runs. But Estabrooks himself had been aiming at high-scoring total results, and these were the sole basis of his conclusion. He made no evaluation of the significance of the other features although he noted them; there was no precedent then known to him for doing so. Many years later, when I looked into his data for the type of results we had been getting in my lab at Duke in somewhat similar ESP tests (e.g., psi missing and declines), I found the negative deviation of hits in his fourth series to be significant. Also, the decline in the runs throughout all four of his series was quite definitely significant in the top-bottom difference in scoring rate. This had become a standard way of evaluating decline effects, but that was long after the date of the experiment. It seems quite reasonable to think that neither Estabrooks nor his subjects could have been motivated to fabricate these peculiar results; rather they were safely attributable to the ESP process itself and in time came to be recognized as typical signs of psi.

Other variations, too, have been found that depend on peculiarities of the psi process overlooked by the original experimenter. One of the most complex examples of this third type is that of the PK record book of M.P.R., a Guilford College student of psychology (Rhine & Reeves, 1943). This experimenter's original tests of dice throwing were self-tests, not intended for publication: but because they were faithfully recorded in a very systematic way and were strictly routine, they were reexamined years after the experiment was over when a search was being made for position effects at the Parapsychology Laboratory at Duke. A hit-distribution analysis conducted by Betty Humphrey was applied to six breakdowns of the data in which decline effects could be expected on the basis of other, previously analyzed work. Of these six breakdowns, five showed the significant differences expected (for example, the top-bottom difference of the hits in the run). The consistency of the patterning was unquestionably a remarkably evidential effect which, under the circumstances, ruled out any conceivable question of dishonesty in the original tests. An independent analysis followed Humphrey's, and others could still be repeated. (The nature of the records was such that trickery in the analysis would almost surely have been discovered. For example, erasures and other alterations are detectable by the experienced analyst, and the records were in a bound composition book.)

Fourth, by far the most imposing body of evidence of the fraud-proof type also occurred in the PK branch of the field. By 1944–45 we had accumulated a large number of research reports from our own laboratories and were examining them for methods of appraisal that would get beyond the great variety of conditions under which they were conducted. The most common feature among all 24 of these reports was the record sheet, a more-or-less standard form on which the results of dice throwing by one means or another could be recorded for the most part in regularly structured rows or columns

on the page. In many experiments the page was divided into four, six, or eight subunits or sets. Eighteen of these reports had pages that were uniform enough to be subdivided into quarters, sometimes omitting median runs, columns, or sets to get uniformity. Twelve of the 18 had these regular sets on the record page.

This afforded a substantial common base of hit distributions with enough uniformity for generalization, and the expected declines in the column or row from the first to the last trial could be measured both ways by the quartering of the page (and the set). The quarter distribution (QD) of the page then became the basis of appraising hit distributions in the 18 series, and an *independent QD analysis* could be applied to the sets in 12 out of the 18. The test of significance to be applied was the difference between the upper left and the lower right quarters, both in the page QD's and the set QD's. These diagonal declines, as they were called, proved to be highly significant and remarkably consistent. Sixteen of the 18 series showed the decline and the *declines were even more significant in the set* than on the page. The point here, of course, is that none of these original experimenters back in the earlier years when the tests were conducted had any idea such an analysis would ever be made. Most of the series had shown significant total scoring above the chance mean. About those results, in some cases more than others, we could have raised questions of accuracy of recording, of bias in the dice, and of unreliability in the experimenters. But after these QD analyses were made, no human weakness or bias in the apparatus could be found that could have produced these declines. Again, Dr. Humphrey did most of the analyses; but prior to issuing an invitation to qualified outside observers to repeat them, I invited Dr. J.G. Pratt to the Laboratory to make a complete independent analysis. Pratt's analysis confirmed Humphrey's very closely.

At the Duke lab we recognized the fraud-proof conclusiveness of the QD analyses; but in the decade that passed before the fraud issue was first raised in a big way by G. Price (1955), the passage of time had allowed the QD's to be largely forgotten. They were not taken seriously by the critics. Yet, meanwhile, they have lost nothing of their antifraud potential.

These four kinds of special evidence against psi fraud do not exhaust the list of possibilities by any means; but they will, I think, suffice to show that this approach to a final settlement of the establishment of psi occurrence has a conclusiveness of its own. We need not leave the contest to the experimental attempts at fraud control where possibilities of the experimenter's motivation to deceive will also have to be dealt with. I am reasonably sure these fraud-proof types of evidence, if now given precedence for a showdown on the establishment of psi, will fill the need for the scientific mind that considers it. But it will take a thorough effort to draw that issue forcefully. Again, however, it must be repeated, this is not to abandon the effort to build up the control system that will eliminate experimenter deception along standard

experimental lines. The evidence of hidden signs of psi, while more conclusive when it applies, is less adaptable (at least as yet) to all test situations. So for the present, we need both lines of control against fraud.

Once the case for psi is conclusive on the basis of the hidden type of evidence, there should be changes welcome to all. Beyond doubt the revolutionary character of psi makes decisive proof exceedingly hard to produce, in contrast to that in the physical sciences. Since there is already some evidence that the subject's belief in psi occurrence affects his test performance, this should be important to the scoring levels. Perhaps experimenters, too, would be affected by a generally relaxed acceptance of the fact of psi ability. We might anticipate that in time there would be little more need for concern about honesty than in any of the other psychological branches of testing and research. Most helpful, perhaps, would be the reduction of skepticism and indifference on the part of those who should be looking into the bearing of psi on their own fields. The future of psi research depends more on this sharing of interest from other disciplines and sciences than upon anything else.

The whole program of parapsychology should be expected to respond to the confidence springing from the assurance that the claim of psi has passed all its tests and that there are no last details which can provide the scientific mind with an excuse even for suspended judgment. More confident steps should then be possible to provide parapsychological education of the highest quality, to obtain adequate economic support, and even to consider practical application over a wide range of possibilities. All this may come in time; but the timing itself may endanger the outcome if left to the unguided circumstances of evolution. So I would say that, with a full appreciation of the major impact of the W.J.L. exposé, it would be best not to forget it until it has spurred us all to clear the terminal barrier to the goal of ultimate establishment.

Notes

1. The first report (Rhine, 1974b) used "Dr. W." as the public identification of Dr. Levy in the slender hope of protecting the interests of innocent people in Levy's nonparapsychological circles. The wide general publicity anticipated at that time can be assumed to have passed. The use of Levy's full name is necessary now, of course, in order to identify his work, which has all been disqualified.

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Price, G. "Science and the Supernatural." *Science*, August 26, 1955, p. 359.

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Rhine, J.B. "Psi Methods Reexamined." *Journal of Parapsychology*, 1975, 39, 38–58.

Rhine, J.B., & Reeves, M.P. "The PK effect: II. A study in declines." *Journal of Parapsychology*, 1943, 7, 76–93.

Suggested Readings

As noted in the general introduction, this is not intended as anything like a complete bibliography. The works mentioned are generally standard sources, for the most part easily available, which may be of help at the next stage of the reader's research.

The case for parapsychology:

Murphy, Gardner. *The Challenge of Psychical Research: A Primer of Psychical Research.* New York: Scribner's, 1966.

Pratt, J. G. *ESP Research Today: A Study of Developments in Parapsychology since 1960.* Metuchen, N.J.: Scarecrow Press, 1973.

Rhine, J. B., and Pratt, J. G. *Parapsychology: Frontier Science of the Mind.* Springfield, Ill.: Charles C. Thomas, 1957.

Wolman, B. B., ed. *Handbook of Parapsychology.* New York: Van Nostrand Reinhold, 1977.

The *Journal of Parapsychology* is also an unequaled source.

Critical works:

Hansel, C. E. M. *ESP: A Scientific Evaluation.* New York: Scribner's, 1966.

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Philosophical treatments:

Braude, Stephen. *ESP and Psychokinesis: A Philosophical Examination.* Philadelphia: Temple University Press, 1979.

Brier, Bob. *Precognition and the Philosophy of Science.* New York: Humanities Press, 1974.

Flew, Antony. "Precognition." In *The Encyclopedia of Philosophy*, ed. Paul Edwards. New York: Macmillan and the Free Press, 1967.

French, Peter, ed. *Philosophers in Wonderland.* St. Paul, Minn.: Llewellyn, 1975. An anthology including philosophical classics in the area.

Ludwig, Jan, ed. Philosophy and Parapsychology. Buffalo, N.Y.: Prometheus Books, 1978. Another good anthology, overlapping to some extent with Peter French's.

Mundle, C. W. K. "ESP: Philosophical Implications." In Edwards, *Encyclopedia of Philosophy*.

On reverse causality, see especially:

Chisholm, R. M., and Taylor, Richard. "Making Things to Have Happened." *Analysis* 20 (1960): 73–78.

Dummett, Michael. "Bringing about the Past." *Philosophical Review* 73 (1964): 338–359.

Dummett, Michael, and Flew, Antony. "Can an Effect Precede Its Cause?" *Proceedings of the Aristotelian Society*, supp. vol. 28 (1954): 27–62.

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SECTION IV

UFOs AND ANCIENT ASTRONAUTS

Introduction

Late in June 1947, Kenneth Arnold told an editor of the *Eastern Oregonian* that he had seen nine thin round aircraft in the vicinity of Mount Rainier, traveling at incredible speeds. He compared their erratic flight to that of saucers skimmed over water, and "flying saucers" immediately became a media sensation.

Thousands of people, of varying backgrounds and in a wide range of circumstances, have reported a boggling assortment of flying and hovering objects, lights in the night sky, and the like. In a perhaps unsuccessful attempt at neutral terminology, these are commonly spoken of as Unidentified Flying Objects. A small sample of fairly typical reports, taken from those labeled "unexplained" within the Condon Report, forms the first selection of this section.

What are UFOs? In the second piece included here, PHILIP J. KLASS maintains that "UFOs are many things, including hot-air or weather balloons, meteor/fireballs, space debris, aircraft conducting unusual flight-test maneuvers, clusters of fireflies, celestial bodies seen through haze and clouds, and outright hoaxes." In this vein Klass carefully discusses three intriguing UFO cases and attempts to "debunk" each of the three, offering in each case an explanation far less dramatic than extraterrestrial vehicles and interplanetary visitors. But Klass also has a general point to make: that the popular image of flying saucers may lead observers of unfamiliar phenomena to give distorted reports, supplying imagined details and interpreting perceptual ambiguities so as to accord with the standard conception of flying saucers.

Much of the debate over UFOs takes the form exhibited by these first two selections; case by case is applauded by the believers, scoffed at by the skeptics, defended by the believers, and so on. In "A Rational Approach to the UFO Problem," however, T. PATRICK RARDIN offers a quite different

approach that does not turn on any particular purported sighting. Following a critical discussion of J. Allen Hynek's definition of UFOs, Rardin introduces a form of a priori probabilistic argument. The basic form of argument at issue here is introduced elsewhere by Carl Sagan using the example of Santa Claus:

> consider the Santa Claus hypothesis. This hypothesis maintains that, in a period of eight hours or so on December 24–25 of each year, an outsized elf visits fifty million homes in the United States. This is an interesting and widely discussed hypothesis. Some strong emotions ride on it, and it is argued that at least it does no harm. We can do some calculations. For example, suppose that the elf in question spends one second per house. This isn't quite the usual picture—"Ho Ho Ho" and so on—but I imagine he is terribly efficient, and very speedy; that would explain why nobody ever sees him very much. With 10^8 houses he has to spend three years just filling stockings. I've assumed he spends no time at all in going from house to house. Even with super-relativistic reindeer, the time spent in 10^8 houses is three years and not eight hours. This is an example of hypothesis testing independent of reindeer propulsion mechanics or debates on the origins of elves. We examine the hypothesis itself, making very straight-forward assumptions, and derive a result inconsistent with the hypothesis by several orders of magnitude. We would then suggest that the hypothesis is untenable.[1]

Sagan has offered a similar argument, presented and examined in some detail by Rardin, that even one legitimate "sighting" each year would demand an unacceptably large number of launches for each technologically advanced civilization that might be within reach of us. Rardin finds Sagan's argument less than compelling, but offers what he takes to be a stronger argument in a similar vein. As things stand, Rardin concludes, there are good reasons to reject any claims linking UFOs with extraterrestrial intelligence—reasons independent of any meticulous investigation of purported sightings.

We introduced the topic of UFOs by referring to Kenneth Arnold's 1947 report. But it is by no means clear that 1947 marks the beginning of the phenomena at issue. Erich von Däniken has been primary among those who have claimed a variety of evidence for visitation by ancient astronauts. In the piece included here, PASQUAL S. SCHIEVELLA presents the case for ancient astronauts and replies to common critical attacks. In the last piece of the section, MARY VETTERLING-BRAGGIN discusses the claims von Däniken makes for his work and rejects it as pseudoscientific. Of importance to Vetterling-Braggin's treatment are careful considerations of notions of possibility and standards for hypothesis selection taken from the work of Carl Hempel.

As a treatment of the issue of UFOs, the selection as a whole is perhaps most valuable for the range of argument it introduces above and beyond the usual debate over particular cases. Klass's general point regarding misiden-

tification and misdescription in UFO reports touches on general worries regarding theory-laden description and raises the question of how sharp a line can be drawn between observation and theory. Issues of possibility are broached by Schievella and further developed by Vetterling-Braggin, and Rardin's discussion of a priori probabilities offers a not unrelated introduction to a number of issues, some alluded to earlier in the volume, concerning hypothesis selection, confirmation, and demarcation.

Notes

1. Carl Sagan, "UFO's: The Extraterrestrial and Other Hypotheses," in *UFO's: A Scientific Debate*, ed. Carl Sagan and Thornton Page (New York: Cornell University Press, 1972), p. 266. Quoted with permission.

Selected Cases from the Condon Report

The United States Air Force had dealt with UFO reports under Project Sign (1948–1949), Project Grudge (1950–1952), and Project Bluebook (1953–1969). In the fall of 1966 the Air Force selected the University of Colorado to conduct an independent study of the UFO issue, in the belief that a study conducted outside the jurisdiction of the Air Force would be of unusual significance with respect to both scientific interest and popular public concern. The scientific director of the study was Edward U. Condon, past president of the American Association for the Advancement of Science, of the American Physical Society, and at one time head of the National Bureau of Standards. The report itself has come to be known as the Condon Report.

Two years and a half-million dollars later, after reviewing and following up approximately sixty cases in detail, calling occasionally on the services of experts in radar, plasma physics, mirages, photographic analysis, and perception, the report concluded that "nothing has come from the study of UFOs in the past 21 years that has added to scientific knowledge . . . further extensive study of UFOs probably cannot be justified in the expectation that science will be advanced thereby."

The Condon Report has been subjected to attack since its publication by both proponents of UFOs, who accuse it of mishandling data, omitting major pieces of evidence, and grasping at explanatory straws, and by critics of UFOs, who hold that its negative conclusions do not go far enough.

The cases that follow are a selection of those that appear in the report as "unexplained."

—the editor

Case 8
North Central
Summer 1966
Investigators: Hynek, Low

Abstract:

Witness was driving in a rural area in late afternoon, when, he said, a silvery metallic-looking disk with dome, about 30 ft. diameter, descended with wobbling motion into the adjacent valley, hovered just above the ground about 200 ft. from the witness, then took off rapidly with a whooshing sound. Depressions in ground and overturned rocks near landing site were offered as evidence, but may have been caused by animals. The report is unexplained.

Background:

Project Bluebook records showed that the witness, a man employed by the U.S. Immigration Service, had reported a UFO sighting. He had been interviewed in the summer of 1966 by the Director of Operations at Minot AFB, who had visited the reported site of the UFO landing. The interview disclosed the following:

About 5:00 on a cloudy day, the witness was driving about one mile north of a town when bright flashes in a clear patch of sky low in the east caught his attention. He stopped and watched as a bright metallic, silvery object dropped below the horizon and moved down the slope opposite him into the shallow valley. It appeared to be tilted, so that he saw it as a disk. A domelike shape on top could be seen. It was about ten feet above the ground, and moved with a wobbly, "falling-leaf" motion. In its center was a dark spot, like smoked glass, about five feet in diameter, and around it three smaller spots. When it reached the valley floor, it rose about 100 ft. and moved to a small reservoir, where it turned horizontal and hovered for about one minute. Then it moved up-slope to a small field and settled down within a few feet of the ground and about 250 ft. from the witness. Thereafter it slowly tilted back on edge, took off with a whooshing sound, and disappeared rapidly into the clouds. The witness' car radio, which had stopped working during the landing, came back to life.

A visit to the reported "landing" site disclosed nothing of interest except two groups of depressions and approximately ten rocks that had been recently displaced. The three depressions in each group were spaced about 9.5–12.5 ft. apart. The rocks were about one foot in diameter or less. The investigating officer commented that persons familiar with wild game in the area had pointed out that grouse make similar depressions in nesting, and that coyotes and badgers overturn rocks in the manner observed. He noted

also that the witness impressed him as a steady, practical kind of person. He wished no publicity, and said he would deny the story if it got out.

Investigation:

Project Investigator Low and Dr. J. Allen Hynek of Dearborn Observatory, Northwestern University, visited the town in the fall of 1966, interviewed the witness and went with him to the site he had reported. They were able to fill in some details: the witness had seen the discoid object at first about .75 mi. distant: it had approached as close as 100 ft.; there it had hovered about one minute, about ten feet off the ground; then it took off and disappeared in about three seconds. The entire observation of the object had taken about five minutes.

At the site, the investigators noted the depressions and the overturned rocks, but were unable to add anything significant to the earlier report. They learned at Minot AFB that no target corresponding to the sighting had appeared on radar.

Comment:

In the absence of supporting witnesses or unambiguous physical evidence, no significant confirmation of the witness' report could be developed. Like other spectacular one-witness sighting reports, it cannot be verified or refuted.

Case 2
Greenwich
Summer 1956
Investigator: Staff

Abstract:

At least one UFO was tracked by air traffic control radar (GCA) at two USAR-RAF stations, with apparently corresponding visual sightings of round, white rapidly moving objects which changed directions abruptly. Interception by RAF fighter aircraft was attempted; one aircraft was vectored to the UFO by GCA radar and the pilot reported airborne radar contact and radar "gunlock." The UFO appeared to circle around behind the aircraft and followed it in spite of the pilot's evasive maneuvers. Contact was broken when the aircraft returned to base, low on fuel. The preponderance of evidence indicates the possibility of a genuine UFO in this case. The weather was generally clear with good visibility.

Background:

The existence of this very interesting radar-visual case was first brought to the attention of the project staff in winter 1968 by the receipt of an unsolicited

letter from one of the principal witnesses, a retired USAF non-commissioned officer who was the Watch Supervisor at the GCA station on the night in question. This letter is rather well written, and since it forms the most coherent account of this UFO case, it is reproduced below in its entirety.

> Reference your UFO Study: you probably already have this item in your file, but in case you don't, I will briefly outline it and you can contact me for full details if you want them.
>
> I retired (20 years service) . . . from the USAF. I have placed my name, rank, and serial number at the top of the page if you want to check on my authenticity. I was an Air Traffic Controller throughout my service career and utilized radar the last 16 years in the control of Air Traffic. I won't bother listing the types and locations, although I could supply all this if needed.
>
> In 1956, . . . (I can't remember the exact date or month), I was on duty as Watch Supervisor at [GCA A] in the Radar Air Traffic Control Center. It was the 5:00 p.m. to midnight shift. I had either four or five other controllers on my shift. I was sitting at the Supervisor's Coordinating desk and received a call on the direct line (actually I'm not sure which line it was). Anyway, it was [GCA B] calling and the radar operator asked me if we had any targets on our scopes travelling at 4,000 mph. They said they had watched a target on their scopes proceed from a point 30 or 40 miles east . . . to a point 40 miles west of [GCA B]. The target passed directly over [GCA B] RAF Station (Also an USAF Station). He said the tower reported seeing it go by and it just appeared to be a blurry light. A C-47 flying over the base at 5,000 feet altitude also reported seeing it as a blurred light that passed under his aircraft. No report as to actual distance below the aircraft. I immediately had all controllers start scanning the radar scopes. I had each scope set on a different range—from 10 miles to 200 miles radius of [GCA A]. At this time I did not contact anyone by telephone as I was rather skeptical of this report. We were using full MTI on our radar, which eliminated entirely all ground returns and stationary targets. There was very little or no traffic or targets on the scopes, as I recall. However one controller noticed a stationary target on the scopes about 20 to 25 miles southwest. This was unusual as a stationary target should have been eliminated unless it was moving at a speed of at least 40 to 45 knots. And yet we could detect no movement at all. We watched this target on all the different scopes for several minutes and I called the GCA Unit at [A] to see if they had this target on their scopes also. They confirmed the target was on their scope in the same geographical location. As we watched, the stationary target started moving at a speed of 400 to 600 mph in a north, northeast direction until it reached a point about 20 miles north northwest of [A]. There was no slow start or build-up to this speed—it was constant from the second it started to move until it stopped.
>
> I called and reported all the facts to this point, including [B] GCA's initial report, to the . . . Command Post . . . I also hooked in my local AFB Commanding Officer and my Unit (AFCS Communications Squadron) Commander on my switchboard. And there could have been others hooked in also that I was not aware of. I repeated all the facts known to this point and continued to give a detailed report on the target's movements and location. The target made several changes in location, always in a straight line, always at about 600 mph and always from a standing or stationary point to his next stop at constant speed—no build-up in speed at all—these changes in location varied

from 8 miles to 20 miles in length—no set pattern at any time. Time spent stationary between movements also varied from 3 to 4 minutes to 5 or 6 minutes (possibly even longer as I was busy answering questions—listening to theories, guesses, etc. that the conference line people were saying). This continued for some time. After I imagine about 30 to 45 minutes, it was decided to scramble two RAF interceptors to investigate. This was done I believe by . . . Air Force calling the RAF and, after hearing what the score was, they scrambled one aircraft. (The second got off after as I will mention later.)

The interceptor aircraft took off from an RAF Station . . . and approached [A] from the southwest. Radio and radar contact was established with the RAF intercept aircraft at a point about 30 to 35 miles southwest [and] inbound to [A]. On initial contact we gave the interceptor pilot all the background information on the UFO, his (the interceptor's) present distance and bearing from [A], the UFO's (which was stationary at the time) distance and bearing from [A]. We explained we did not know the altitude of the UFO but we could assume his altitude was above 15,000 feet and below 20,000 feet, due to the operational characteristics of the radar (CPS-5 type radar, I believe). Also we mentioned the report from the C-47 over [B] that relayed the story about the light which passed below him. His altitude was 5,000 feet.

We immediately issued headings to the interceptor to guide him to the UFO. The UFO remained stationary throughout. This vectoring of the interceptor aircraft continued. We continually gave the intercept aircraft his heading to the UFO and his distance from the UFO at approximately 1 to 2 mile intervals. Shortly after we told the interceptor aircraft he was one-half mile from the UFO and it was twelve-o'clock from his position, he said, "Roger, . . . I've got my guns locked on him." Then he paused and said, "Where did he go? Do you still have him?" We replied, "Roger, it appeared he got behind you and he's still there." [There were now two targets; one behind the other, same speed, very close, but two separate distinct targets.]

The first movement by the UFO was so swift (circling behind the interceptor); I missed it entirely, but it was seen by the other controllers. However, the fact that this had occurred was confirmed by the pilot of the interceptor. The pilot of the interceptor told us he would try to shake the UFO and would try it again. He tried everything—he climbed, dived, circled, etc., but the UFO acted like it was glued right behind him, always the same distance, very close, but we always had two distinct targets. [Note: Target resolution on our radar at the range they were from the antenna (about 10 to 30 miles, all in the southerly sectors from [A]) would be between 200 and 600 feet probably. Closer than that we would have got one target from both aircraft and UFO. Most specifications say 500 feet is the minimum, but I believe it varies and 200 to 600 feet is closer to the truth and, in addition, the tuning of the equipment, atmospheric conditions, etc., also help determine this figure.]

The interceptor pilot continued to try and shake the UFO for about ten minutes (approximate—it seemed longer both to him and us). He continued to comment occasionally and we could tell from the tonal quality he was getting worried, excited and also pretty scared.

He finally said, "I'm returning to Station [A]. Let me know if he follows me. I'm getting low on petrol." The target (UFO) followed him only a short distance, as he headed south southwest, and the UFO stopped and remained stationary. We advised the interceptor that the UFO target had stopped following and was now stationary about 10 miles south of [A]. He rogered this

message and almost immediately the second interceptor called us on the same frequency. We replied and told him we would advise him when we had a radar target, so we could establish radar contact with his aircraft. (He was not on radar at this time, probably had just taken off and was too low for us to pick him up, or too far away—we had most of the scopes on short range, so we could watch the UFO closely on the smaller range.) The number two interceptor called the number one interceptor by name (Tom, Frank—whatever his name was) and asked him "Did you see anything?" Number one replied, "I saw something, but I'll be damned if I know what it was." Number two said, "What happened?" Number one said, "He (or it) got behind me and I did everything I could to get behind him and I couldn't. It's the damnest thing I've ever seen." Number one also made a remark at this time to number two, that he had his radar locked on whatever it was for just a few seconds so there was something there that was solid. Number one then switched frequencies to his home base frequency. We gave number two the location of the UFO and advised him that we still didn't have him on radar, but probably would have shortly. He delayed answering for some seconds and then finally said, "[A] ———" (Identification aircraft call sign)—can't remember what call sign these aircraft were using. "Returning home, my engine is malfunctioning." He then left our frequency.

Throughout this we kept all the agencies, . . . advised on every aspect, every word that was said, everything.

We then inquired what action they wanted to take. They had no more suggestions and finally they told us to just keep watching the target and let them know if anything else happened. The target made a couple more short moves, then left our radar coverage in a northerly direction—speed still about 600 mph. We lost target outbound to the north at about 50 to 60 miles, which is normal if aircraft or target is at an altitude below 5,000 feet (because of the radiation lobe of that type radar). We notified . . . Air Division Command Post and they said they'd tell everybody for us.

I made out a written report on all this, in detail for the officers in charge of my facility, and was told that unless I was contacted later for further information, he would take care of it. I don't know if a CERVIS report was submitted on this or not—I heard no more about it.

All speeds in this report were calculated speeds based on time and distance covered on radar. This speed was calculated many times that evening and although this happened quite a while ago, the basic elements are correct.

Fig. 1 shows a map of the contact as drawn by the witness.

Investigation:

Since this case was discovered so late in the project, investigation was limited to a follow-up request for additional information from Project Bluebook, and analysis of the available details of the case by investigators familiar with radar and optical propagation anomalies.

Copies of the Project Bluebook files on the case were received in late August of 1968. . . . One of the interesting aspects of this case is the remarkable accuracy of the account of the witness as given in the letter reproduced above, which was apparently written from memory 12 yrs. after the incident. There are a number of minor discrepancies, mostly a matter of figures (the

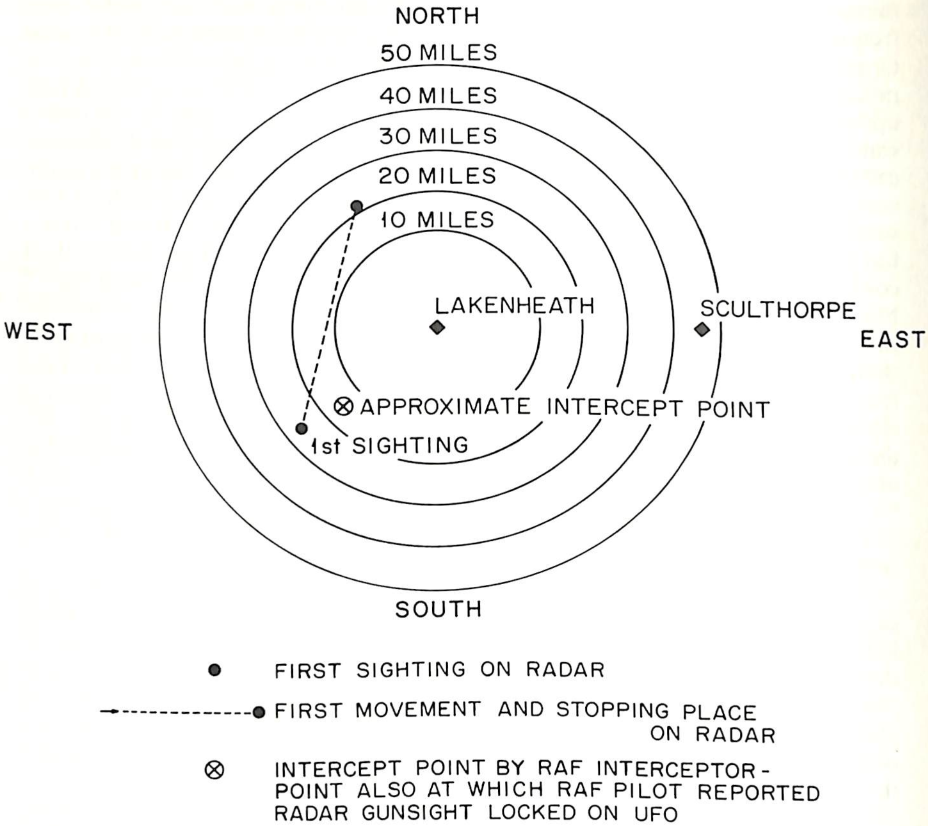


Figure 1.

C-47 at 5,000 ft. was evidently actually at 4,000 ft.), and he seems to have confused the identity of location C with B; however, all of the major details of his account seem to be well confirmed by the Bluebook account. . . .

Conclusions:

In view of the multiple radar sightings involved in this case, any conventional explanation for the occurrences reported would seem to require some sort of radar anomalous propagation. As pointed out in Chapter 7, the evidence for anomalous propagation in this case is rather uncertain. The temporary disappearance of the target . . . is quite suggestive of anomalous propagation. The generally clear weather was conducive to the formation of the atmospheric stratification that causes anomalous propagation, although it by no means follows that such formation would have actually occurred. In this connection, the apparent near-coincidence between the appearance of broken clouds (0330 GMT) and the disappearance of the radar targets (0330 GMT) could be significant.

On the other side must be balanced the generally continuous and consistent movements of the radar tracks reported by [A], which are not at all typical of radar false targets caused by anomalous propagation. In addition, some of the maneuvers reported in the radar controller's letter to have been executed by the UFO are extremely unlikely to be duplicated by a false target, in particular stopping and assuming a new path after following the intercepting aircraft for some time. The comments of the Air Force officer who prepared the UFO message reproduced earlier are also significant.

In an early Air Force investigation it was suggested that the visual sightings might have been caused by the Perseid meteors. However, as Air Force Consultant Dr. Hynek pointed out:

> It seems highly unlikely, for instance, that the Perseid meteors could have been the cause of the sightings, especially in view of the statement of observers that shooting stars were exceptionally numerous that evening, thus implying that they were able to distinguish the two phenomena. Further, if any credence can be given to the maneuvers of the objects as sighted visually and by radar, the meteor hypothesis must be ruled out. . . .

In conclusion, although conventional or natural explanations certainly cannot be ruled out, the probability of such seems low in this case and the probability that at least one genuine UFO was involved appears to be fairly high.

Case 5
South Central
Fall 1957
Investigator: Craig

Abstract:

The crew of a B-47 aircraft described an encounter with a large ball of light which was also displayed for a sustained time for both airborne radar monitoring receivers and on ground radar units. The encounter had occurred ten years prior to this study. Project Bluebook had no record of it. Attempts to locate any records of the event, in an effort to learn the identify of the encountered phenomenon, failed to produce any information. The phenomenon remains unidentified.

Background:

At a project-sponsored conference for air base UFO officers, held in Boulder in June 1967, one of the officers revealed that he personally had experienced a puzzling UFO encounter some ten years previously. According to the officer, a Major at the time of the encounter, he was piloting a B-47

on a gunnery and electronic counter-measures training mission from an AFB. The mission had taken the crew over the Gulf of Mexico, and back over South Central United States where they encountered a glowing source of both visual and 2,800 mHz. electromagnetic radiation of startling intensity, which, during part of the encounter, held a constant position relative to the B-47 for an extended period. Ground flight control radar also received a return from the "object," and reported its range to the B-47 crew, at a position in agreement with radar and visual observations from the aircraft.

According to the officer, upon return to the AFB, electronic countermeasures, graphic data, and radar scope pictures which had been taken during the flight were removed from the plane by Intelligence personnel. He recalled that an Intelligence questionnaire regarding the experience had later been completed by the B-47 crew; however, the "security lid" shut off further information regarding the encounter. The crew learned nothing more regarding the incident, and the pilot occasionally had wondered about the identity of the phenomena encountered ever since his experience.

Investigation:

When no report of this incident was found in Bluebook or Air Defense Command records, this project undertook to obtain leads to the location of data recorded during the event through detailed interview of all available members of the B-47 crew. Of the six crew members, the three most closely involved in the encounter were the pilot, co-pilot, and the officer who had been in charge of the most involved radar-monitoring unit.

Details of the encounter, as best they could be recalled, were obtained by interview with the pilot and, later, with the two other officers at another air base. All remained deeply impressed by the experience, and were surprised that a report of it was not part of Bluebook files. Their descriptions of the experience were generally consistent, although the pilot did not mention that the navigator also had received a radar return from the object in question, as was recalled by the other officers. (The navigator, on duty in Vietnam, was not available for interview). The two other crew members, each of whom had operated a radar monitoring unit in the B-47 during the UFO event, were involved to a lesser extent in the incident, and were not located for interview.

The crew's description of the experience follows:

Time: Early morning, Fall 1957.
Place: Over South Central United States
Plane's altitude: Above 30,000 ft. during the first part of the encounter.
Nature of Mission: (Pilot): Combined navigation, gunnery, and electronic counter-measure training mission.
(Other Crew): Check-out of plane and equipment, including electronic counter-measures equipment, prior to European assignment.

Weather: Witnesses recalled seeing, from 30,000 ft. altitude, lights of cities and burn-off flames at gas and oil refineries below. They have no recollection of other than clear weather.

Radar monitoring unit number two, in the back end of the B-47, picked up a strong signal, at a frequency of about 2,800 mHz., which moved up-scope while the plane was in straight flight. (A signal from a ground station necessarily moves down-scope under these conditions, because of forward motion of the airplane.) This was noted, but not reported immediately to the rest of the crew. The officer operating this unit suspected equipment malfunction, and switched to a different monitoring frequency range. The pilot saw a white light ahead and warned the crew to be prepared for a sudden maneuver. Before any evasive action could be taken, the light crossed in front of the plane, moving to the right, at a velocity far higher than airplane speeds. The light was seen by pilot and co-pilot, and appeared to the pilot to be a glowing body as big as a barn. The light disappeared visually, but number two monitor was returned to the frequency at which the signal was noted a few moments earlier and again showed a target, now holding at the "two-o'clock" position. The pilot varied the plane's speed, but the radar source stayed at two o'clock. The pilot then requested and received permission to switch to ground interceptor control radar and check out the unidentified companion. Ground Control in the area informed the pilot that both his plane and the other target showed on their radar, the other target holding a range of ten miles from him.

After the UFO had held the two o'clock position and ten-mile range through various test changes in aircraft speed, the number two monitoring officer informed the pilot that the target was starting to move up-scope. It moved to a position dead ahead of the plane, holding a ten-mile range, and again became visible to the eye as a huge, steady, red glow. The pilot went to maximum speed. The target appeared to stop, and as the plane got close to it and flew over it, the target disappeared from visual observation, from monitor number two, and from ground radar. (The operator of monitor number two also recalled the B-47 navigator's having this target on his radar, and the target's disappearing from his radar scope at the same time.) The pilot began to turn back. About half way around the turn, the target reappeared on both the monitor and ground radar scopes and visually at an estimated altitude of 15,000 ft. The pilot received permission from Ground Control to change altitude, and dove the plane at the target, which appeared stationary. As the plane approached to an estimated distance of five miles the target vanished again from both visual observation and radar. Limited fuel caused the pilot to abandon the chase at this point and head for his base. As the pilot leveled off at 20,000 ft. a target again appeared on number two

monitor, this time behind the B-47. The officer operating the number two monitoring unit, however, believes that he may have been picking up the ground radar signal at this point. The signal faded out as the B-47 continued flight.

The co-pilot and number two monitoring officer were impressed by the sudden disappearance of the target and its reappearance at a new location. As they recalled the event, the target could be tracked part of the time on the radar monitoring screen, as described above, but, at least once, disappeared from the right side of the plane, appeared on their left, then suddenly on their right again, with no "trail" on the radar scope to indicate movement of the target between successive positions.

The monitoring officer recalled that the navigator, who reported receiving his own transmitted radar signals reflected from the target, not only had a target on his screen, but reported target bearings which coincided exactly with the bearings to the source on the monitoring scope. He also indicated that the officer operating the number one radar monitoring unit, which was of a different type, having a fixed APD-4 antenna instead of a spinning antenna as used with the number two unit, and covering all radar ranges, also observed the same display he observed on unit two. The sixth crew member, operating number three radar monitor, which covered a lower frequency range, was searching for something to tie in with the signals being observed on the other scopes, but found nothing.

The following questions are raised by this information:

1) Could the number two monitoring unit have received either direct or reflected ground radar signals which had no relation to the visual sighting?

The fact that the frequency received on number two, about 2,800 mHz., was one of the frequencies emitted from ground radar stations (CPS6B type antennas) at an airport and other airports nearby, makes one suspect this possibility. The number two monitoring officer felt that after the B-47 arrived over South Central U.S., signals from GCA sets were received, and this confused the question of whether an unidentified source which emitted or reflected this wave length was present. On original approach to the area, however, a direct ground signal could not have moved up-scope. Up-scope movement could not have been due to broken rotor leads or other equipment malfunction, for all other ground signals observed that night moved down-scope. A reflected signal would require a moving reflector in the region serving as apparent source, the movement being coordinated with the motion of the aircraft, particularly during periods when the UFO held constant position relative to the moving aircraft. Since the monitor scans 360°, if a reflected beam were displayed on the scope the direct radar beam also would be displayed, unless the transmitter were below the horizon. As the event was recalled by the witnesses, only one signal was present during initial observations. If the UFO actually reflected radar signals transmitted from the

B-47, and appeared in the *same* position on the navigator's scope as on the number two monitoring scope, reflection of 2,800 mHz. ground signals from these same positions seems extremely unlikely.

2) Could the visual observations have been misinterpreted airplane lights, airplane afterburners, or meteors?

The persistence of the phenomenon rules out meteors. Observed speeds, plus instant re-position and hovering capabilities are not consistent with the aircraft hypothesis.

3) Were the visual observations necessarily of the same phenomenon as the radar observations?

Coincidence of disappearances, appearances, and indicated positions suggests a common cause.

4) If the reported observations are factual and accurate, what capabilities and properties were possessed by the UFO?

a) Rapid motion, hovering, and instant relocation.

b) Emission of electromagnetic radiation in the visible region and possibly in the 2,800 mHz. region.

c) Reflection of radar waves of various frequencies (from airborne radar units as well as 2,800 mHz. ground units). Failure to transmit at the frequency of the number three radar monitor.

d) Ability to hold a constant position relative to an aircraft.

5) Could the observed phenomenon be explained as a plasma?

Ten scientists who specialize in plasma research, at our October 1967 plasma conference regarded an explanation of this experience in terms of known properties of a plasma as not tenable.

Further investigation of this case centered around efforts to trace reports of this event submitted by the crew after the B-47 returned to the AFB. Recollections of the nature and manner of submission of such reports or records were in sharp divergence. As the pilot recalled the incident, the landing plane was met by their Wing Intelligence personnel, who took all filmed and wire-recorded data from the "back-end" crew. The crew was never extensively questioned about the incident. Days or weeks later, however, the crew did receive from Air Defense Command, a lengthy questionnaire which they completed including sketches of what they had seen and narrative descriptions of the event. The questionnaire also had a section to be completed by the ground radar (GCI) personnel. The pilot could not recall where or exactly when the completed questionnaire had been sent.

In contrast with this recollection, the co-pilot and number two monitoring officer said that no data whatsoever had been recorded during the flight. The #1 monitoring unit was equipped for movie filming of its display, and #2 was equipped for wire recording of data. Since the flight had been merely for the purpose of checking equipment, however, neither film nor recording wire was taken aboard. Both these officers recalled intensive interrogation by their

Intelligence personnel immediately after their return to the AFB. They did not recall writing anything about the event that day or later. According to their account, the B-47 crew left for England the following day, and heard nothing more of the incident.

Since it appeared that the filmed and recorded data we were seeking had never existed, we renewed the effort to locate any special intelligence reports of the incident that might have failed to reach Project Bluebook. A report form of the type described by the pilot could not be identified or located. The Public Information Officer at ADC Headquarters checked intelligence files and operations records, but found no record of this incident. The Deputy Commander for Operations of the particular SAC Air Wing in which the B-47 crew served in 1957 informed us that a thorough review of the Wing history failed to disclose any reference to an UFO incident in Fall 1957.

Conclusion:

If a report of this incident, written either by the B-47 crew or by Wing Intelligence personnel, was submitted in 1957, it apparently is no longer in existence. Moving pictures of radar scope displays and other data said to have been recorded during the incident apparently never existed. Evaluation of the experience must, therefore, rest entirely on the recollection of crew members ten years after the event. These descriptions are not adequate to allow identification of the phenomenon encountered. . . .

UFOs: Fact or Fantasy?

PHILIP J. KLASS

For more than twenty-five years, thousands of persons in the United States and around the globe have reported seeing strange seemingly unexplainable things in the sky, or coming from the sky. Perhaps you yourself, or a respected friend, have had such an experience.

Some skeptics dismiss UFOs out of hand because they think that UFO reports only come from "kooks" and "little old ladies in gym shoes." This simply is not true, as we shall see in the cases to be discussed here.

On the other hand, there are some—including a few scientists with impressive-sounding academic credentials—who are convinced that some UFO reports involve extraterrestrial spaceships. Yet, despite more than a quarter-century of UFO reports, there is not a single piece of physical evidence—not a single authentic artifact or photograph—to support the hypothesis that UFOs are of extraterrestrial origin.

Several dozen persons have claimed to have been aboard "flying saucers" for extended periods of time. Yet, despite the fact that we humans are great collectors of souvenirs, not one of these persons has brought back so much as an extraterrestrial tool or artifact, which could, once and for all, resolve the UFO mystery.

And so the extraterrestrial hypothesis rests entirely on reports, by seemingly intelligent, seemingly honest citizens, of unusual-looking objects that appear to do strange things.

When such UFO reports come from pilots, many people accept them as gospel in all their details, and we will shortly consider such cases. Another kind of UFO report that carries high credibility are those that involve multiple witnesses, especially when the witnesses are unknown to one

another and are widely separated geographically at the time of the UFO sighting.

Just such an incident occurred during the evening of March 3, 1968, at approximately 8:45 p.m. eastern standard time. The mayor of Nashville, Tennessee, a member of his staff, and the latter's wife were standing outside talking, when the woman spotted a curious light moving toward them at high speed. She called it to the attention of the two men, and all three turned their attention to the object.

As the UFO came closer, according to a very detailed, written report that the woman later submitted to the U.S. Air Force, the observers could see orange flames shooting out from behind the UFO. Soon it was passing directly overhead—in eerie silence. The woman's report said the UFO was shaped "like a fat cigar—the size of one of our largest airplane fuselages, or larger."

The woman reported that the giant craft had at least ten square-shaped windows that were brightly illuminated from inside. She also observed: "It appeared to me that the fuselage was constructed of many pieces of flat sheets of metal-like material with a riveted-together look." The woman drew a sketch of the UFO (See Figure 1.)

In her report to the USAF, the woman said that, after the object had passed, the three adults discussed what they had seen and speculated that the object must have been either a top-secret military airplane or "a craft from outer space!"

The USAF also received a report from persons near Shoals, Indiana, approximately two hundred miles north of Nashville. A group of six persons

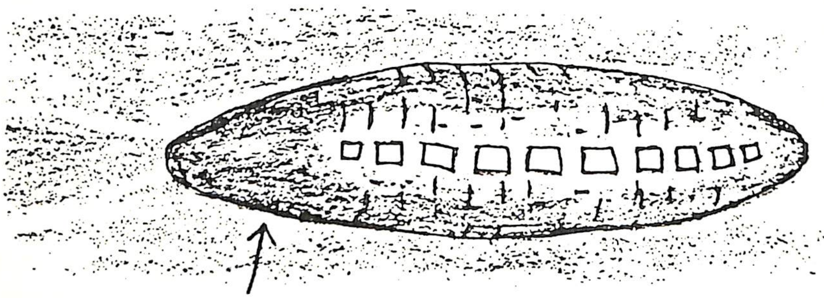


Figure 1.



Figure 2.

Sketches submitted by observers in Tennessee (Fig. 1) and Indiana (Fig. 2) of UFO sighted on March 3, 1968. The incident resulted from flaming pieces of debris from a Russian rocket that reentered the atmosphere, becoming in effect a "man-made meteor."

there also reported seeing a glowing cigar-shaped object with many brightly illuminated windows on the night of March 3, at approximately the same time as the Nashville sighting. (See Figure 2.)

That same night, a business executive from Dayton, Ohio, was returning home in his car from Columbus when he saw what appeared to be three UFOs flying in formation, with one in the lead and two behind. He put down the details in a letter sent to Dr. John Keto, then the technical director of the USAF's Wright-Patterson Air Force Base in Dayton. In this letter, the business executive said that he knew that the USAF probably received a lot of UFO reports from persons suspected of being "kooks," but he added that since he and Dr. Keto had been friends for many years he would depend on Keto to testify to the fact that the executive was not a "kook." His letter reported that when the lead UFO appeared to turn, or maneuver, the other two followed. In the UFO literature, this is referred to as evidence of "intelligent control," suggesting that UFOs are under the control of some form of intelligent beings.

The USAF also received a written report from a school-teacher who lived near Columbus, Ohio, describing her own UFO sighting on the same night. The woman said she was a science teacher and had four academic degrees, including a Ph.D. She had been out walking her dog when she suddenly spotted a squadron of three UFOs, according to her account, flying in formation.

The woman was carrying binoculars, which she trained on the UFOs. She reported to the USAF that the objects were shaped like "inverted saucers," which is a familiar description of UFOs in the literature. The school teacher reported that the UFOs had a mysterious effect on her dog, who was whimpering as if she were "frightened to death."

But the UFOs had an even more mysterious effect on the woman herself, as she recounted in her letter. Upon returning to her house, she had an overpowering urge to sleep and could not keep her eyes open, she wrote. She added that this was surprising because she had slept a full ten hours the night before, had taken a nap that afternoon, and wanted to stay awake to receive an expected telephone call. She said she opened the windows wide to let in the cool night air, but despite this, she could not stay awake. She slept until her friend called, by which time the strange UFO-induced sleepiness had completely disappeared. It was as if some strange force emitted by the UFOs had mesmerized this mature, well-educated woman.

Several months later, on the night of July 7, 1968, a pilot of a private plane, returning home to Seattle, had a frightening encounter with a squadron of *nine* UFOs. As the pilot later described the incident, he had been headed west when he spotted nine ruby-red lights that seemed to be flying directly toward him on a collision course. To avoid a collision, the pilot made a sharp turn to his right, and to his amazement the nine ruby-red UFOs seemed to turn to

their right. Obviously, the UFOs did not want a collision, and their action was seen as evidence of "intelligent control."

The pilot decided he had encountered a formation of military jet aircraft. But when he called the Seattle tower, the pilot was told that there were no known military aircraft in the area. It was a clear, moonlit night, and the pilot put his airplane into a turn so he could circle and watch the UFOs. Shortly, as the pilot later reported, he saw the UFOs begin to fire rockets earthward, as if the UFOs were attacking Seattle itself.

Suddenly, to the pilot's horror, he noticed that the UFOs once again had started heading for his airplane. In desperation he turned on his landing lights to warn the UFOs of his presence. He reported that the UFOs once again exhibited intelligent control by stopping their headlong attack and appearing to back up.

Finally, one by one, the UFOs began to disappear, until the last one had vanished; then the badly shaken pilot headed for the Seattle airport. But during his final approach, he discovered that the airplane's gyrocompass was 170° in error, that is, almost completely backward. And the clock on the cockpit panel had stopped at 10:35 p.m.! It seemed to the pilot that the UFOs had emitted mysterious radiation that could introduce gross errors into a gyrocompass and stop a clock. The badly shaken pilot later said: "I wouldn't believe it myself, but I saw it."

Nearly a year after the Seattle incident, on June 5, 1969, shortly before 6 p.m., under conditions of broad daylight and unlimited visibility, two airliner crews and a military jet pilot had a frightening encounter with a squadron of UFOs. American Airlines Flight #112, which had departed San Diego several hours earlier and was now headed east en route to Washington, D.C., was cruising at thirty-nine thousand feet over St. Louis.

The captain had gone back to the passenger cabin and his seat was occupied by a senior air-traffic controller from the Federal Aviation Administration, who was flying as a cockpit observer. Suddenly the co-pilot hollered, "Damn, look at this." The FAA controller looked, and as he later described it, "There it was—a flight of four whatever-they-were flying in a square formation." There was a larger, hydroplane-shaped UFO flying in the lead, followed by three smaller objects, all headed west. *They seemed to be on a near-collision course with the jetliner!* The objects seemed to be propelled by rocket engines that emitted long tails of blue-green flame.

Moments before the squadron of UFOs would have collided with the jetliner, they appeared to exhibit "intelligent control" and to take evasive action. Despite this, the FAA controller estimated that the objects had come within 300 feet of the airliner!

The co-pilot radioed down to the nearby St. Louis tower to ask if it had any unidentified targets on the airport radar. Soon a controller called back to say

that there were two unidentified targets west of the airliner, which seemed to confirm the visual sighting.

A few miles west of the American Airlines jetliner was an eastbound United Airlines jetliner cruising at thirty-seven thousand feet, and its crew had monitored the radio conversation between the American Airlines crew and the St. Louis tower. Shortly, the United crew radioed, "We see it, too."

A few miles west of the United Airlines aircraft was an eastbound Air National Guard jet fighter plane, cruising at forty-one thousand feet, whose pilot also had monitored the radio communications with the St. Louis tower. A few moments later, he broke in to report, "Damn, they almost got me." He went on to say that the squadron of UFOs seemed to be headed directly for his airplane, until the last moment, when they seemed to abruptly change course to climb out of his path. Here is still another report that the objects, whatever they were, were under "intelligent control."

These three high-quality UFO reports are only a sample of the best of the thousands of UFO cases on file. Intelligent, well-educated observers, including experienced airline and military pilots, reported behavior of UFOs that they thought clearly indicated that the objects were under "intelligent control." Additionally, there was the independent radar confirmation by the St. Louis tower. Also there was the mysterious UFO-induced effect on the teacher's dog, which resembled other cases in which the presence of UFOs was reported to have caused fright in animals. And in the Seattle case, we even have what appeared to be UFO-induced effects on inanimate objects—the gyrocompass and aircraft clock.

If I were the typical UFO lecturer, who usually receives from five hundred to a thousand dollars, or more, for a one-hour lecture, I would continue to amaze you with a procession of such seemingly mysterious and unexplainable cases—unexplainable except in terms of extraterrestrial visitors whose technology is so advanced that it necessarily seems like black magic to earthlings with a primitive understanding of science.

But having been trained as an engineer, having worked for more than two decades as an investigative technical journalist, and being a skeptic by nature, I find it more interesting to seek prosaic explanations for seemingly mysterious events. Let us now reexamine these three UFO incidents.

Returning to the incident of March 3, 1968—from the reported time of the observations in Nashville, Shoals, Dayton, and Columbus, in which the object, or objects, all were reported to be coming out of the southwest and headed northeast, it is clear that all of the observers were seeing the same thing. But was it one giant saucer/cigar-shaped object with square illuminated windows, as reported by the three observers near Nashville and six witnesses near Shoals, or was it really three UFOs flying in formation, as described by the business executive and science teacher?

Many UFO investigators would automatically conclude that democratic procedures should be used and that, with a total of nine observers reporting a single large object with illuminated windows and only two reporting three smaller objects, the majority must be correct. In reality, it is possible to say with certainty that in this instance the majority was wrong.

We know what it was that all of these observers, as well as dozens of other observers extending as far as western Pennsylvania, were seeing, because the North American Air Defense Command keeps tabs on objects in space, using a far-flung global network of powerful radars. We now know that the UFO(s) reported on the night of March 3, 1968, at approximately 8:45 p.m. EST, were flaming metal fragments from a Russian rocket that had been used to launch the Zond-4 spacecraft on a translunar-type trajectory. When that large rocket lost sufficient altitude to encounter dense air in the atmosphere, it "re-entered," breaking up into metal fragments that were heated to incandescence by the high speed of re-entry.

The witnesses near Nashville and Shoals were unwitting victims of their own imagination and what they had read or heard about UFOs. When the observers sought to explain the flaming objects that were unlike anything they had seen before, they concluded that the objects must be the windows of a giant craft, illuminated from inside. Since the "craft" made no noise, it could not be an airplane. Therefore, it must be a UFO. If it was a UFO, then it must be saucer- or cigar-shaped, and the imagination obligingly supplied nonexistent details about the fuselage, such as the metal-riveted-together look, *which the eyes never saw!*

The science teacher and business executive were much more accurate in their descriptions. The apparent maneuver of the lead object and those trailing behind most probably was an optical illusion. Certainly there was no "intelligent control" involved in the trajectory of the flaming fragments of rocket body.

But how can we explain the strange after-effect reported by the science teacher? As the woman explained in her letter, she had long been interested in UFOs and had once seen what she thought to be a UFO. This is why she always carried binoculars, when walking here dog, in the hope of getting a closer look if she should ever be lucky enough to have a second sighting.

On the night of March 3, she was favored with the good luck to see not one but *three* UFOs. When she viewed them through the binoculars, her brain unwittingly supplied the "inverted-saucer shape," about which she had read in UFO books. When she returned home, she was so excited by her good fortune to have seen three spaceships from a distant world—that she was probably overwhelmed emotionally and became drowsy.

But what about the dog? Clearly the dog had not read any articles or books on UFOs. The answer can be found in the woman's own letter. At one point she mentioned that the temperature was "clear and cold," approximately

eight degrees below freezing. Elsewhere in her letter she mentioned that the dog *hated* cold weather. The science teacher had stopped walking her dog to view the UFOs with her binoculars, and also to try to communicate with the UFOs by means of her flashlight. The dog must have become chilled and whimpered simply because she wanted to return to a warm house.

Let us now turn to the Seattle UFO incident. If you had been a resident of the area on July 8, 1968, you would have learned of the event from the banner headline and front-page story in the morning newspaper. But the following morning, if you had looked for further news of the exciting incident, expecting to find it on the front page, you would have found nothing. Apparently, there had been no new developments.

If, however, you were a very observant and careful reader, you would have found an explanation for the exciting events *buried on page twelve.* The squadron of UFOs that had terrified the pilot and some local citizens by the "attack" on Seattle had been produced by several teen-age boys, who admitted the hoax.

They had used plastic garment bags to fashion hot-air balloons by installing candles in the lower opening after sealing up the top. The candles provided both the hot air needed for flight, and illumination to make the objects visible. To add excitement, they had attached railroad flares with long fuses so that the flare would not ignite until sometime after the balloons became airborne. When the flares finally ignited and burned free, falling to the ground, they became the "rockets" that the pilot and ground observers had reported seeing.

But what explanation is there for the seemingly "intelligent reaction" of the UFOs to the pilot's evasive maneuver, and later to his turning on his landing lights? Clearly the balloons were simply responding to the wind conditions aloft. In the darkness, it would be difficult for a frightened pilot to determine the precise movements of the objects.

The explanation for the 170° error in the gyrocompass also is easy to explain. Private planes necessarily carry relatively inexpensive instruments. As any experienced pilot knows, and as any gyroscope designer can tell you, if an aircraft flies in circles for an extended period of time with a gyrocompass that is not "slaved" to magnetic north—a feature found only in more expensive instruments—the instrument will experience what is called "gyro turn error." Recall that the Seattle pilot was flying in circles for some time, to better watch the UFOs. In so doing, his gyrocompass simply accumulated "gyro turn error."

The explanation for the stopped clock in the cockpit is even simpler. Cockpit clocks must be hand-wound to keep running. If a pilot becomes occupied watching a squadron of UFOs and forgets to wind his clock, it will stop running.

These two cases demonstrate that intelligent, well-educated observers can be grossly in error in trying to appraise what they have seen when the incident occurs in darkness. But what of the St. Louis incident, in which the encounter occurred in broad daylight and was seemingly confirmed by radar, and in which the observers were experienced flight crews?

In this case, it is possible to determine with certainty what it was that these experienced flight crews observed, because it was photographed by an alert newspaper photographer named Alan Harkrader, in Peoria—125 miles north of St. Louis!

The large "UFO" in the lead was really a giant flaming meteor/fireball, coming out of the east and headed west, which also was seen by numerous ground observers in central Illinois and Iowa. Flaming fragments periodically would break off and fall into trail, to form a "squadron." From these many ground observations and the photo taken in Peoria, scientists have been able to establish the approximate flight path of the fireball and its trailing flaming fragments.

The trajectory of the fireball and its fragments was *125 miles* north of the three aircraft flying near St. Louis whose crews believed that the objects almost collided with their aircraft! Two pilots at the airport in Cedar Rapids, Iowa, also saw the fireball and fragments. They reported the incident immediately to the Federal Aviation Administration office at the airport. According to these pilots, the flaming objects passed directly over the east-west runway of the airport at an altitude of only one thousand feet. In reality, the fireball path was *100 miles* to the south! And we know the objects' altitude was something above forty-one thousand feet, the altitude of the Air National Guard aircraft whose pilot reported the UFOs had passed above him.

But what of the seeming confirmation provided by the St. Louis tower that its radar showed two unidentified objects west of the American Airlines jetliner? While meteors can be detected by radar, the fireball and fragments were too far away to be detected by the St. Louis radar.

At the time of this incident—in 1969—the radar displays in the St. Louis tower had not yet been modernized and could not *automatically* display the identity, and altitude, of all aircraft in the area that were equipped with "radar transponders." In 1969, aircraft that were over-flying the St. Louis airport (that is, that did not intend to land there) were not required to inform the tower of their identity. Only those airplanes that were coming in to land at St. Louis would be identified on the radar display, by means of small plastic markers on which the controllers had scribbled airplane identity and altitude obtained from voice radio reports.

Thus, when the St. Louis tower controller heard that a squadron of UFOs coming out of the east had passed to the west of the American Airlines jetliner, which now was over St. Louis, the controller hastily examined his

radarscopes and indeed found two unidentified targets west of the airliner. These almost certainly were the United Airlines jetliner and the Air National Guard airplane—which would not have identity markers since they were not coming in to land at St. Louis. But in the excitement of the moment, these unidentified blips became UFOs!

These three cases are not the most difficult and challenging ones that I have tackled, and explained, in the past decade.

But they do illustrate the all-important principle that even the most intelligent of observers, including experienced flight crews, can be grossly in error in trying to describe what they think they have seen and experienced.

In the final analysis, the question of what UFOs are and where they come from can be answered as follows.

UFOs are many things, including hot-air or weather balloons, meteor/fireballs, space debris, aircraft conducting unusual flight-test maneuvers, clusters of fireflies, celestial bodies seen through haze and clouds, and outright hoaxes. There are dozens of potential stimuli to be seen in the skies, especially at night.

Given these many stimuli, it is clear that UFOs "come from" the viewer's own imagination when he or she sees something unusual, especially at night, because many of us have been unwittingly brainwashed by the biased coverage given to UFOs by the news media, especially by one national television network.

After a decade of investigating the most challenging UFO cases—the ones that have been hailed by "believers" and have won prizes as being the most mysterious and inexplicable—I have yet to encounter one that would make me suspect that we might have had even a single extraterrestrial visitor.

How confident am I of this appraisal? Having been born and raised in the Midwest, I believe in that expression that was popular when I was a youth: Talk is cheap; put your money where your mouth is. In the closing chapter of my recent book *UFOs Explained* (Random House), I have done just that. I offer to buy back every copy of the book at its list price of $8.95 if at any time evidence of an authentic extraterrestrial spaceship is ever found. If one lands in Buffalo, or crashes in London, or a piece of one falls off and is pronounced by the respected National Academy of Science to be truly an extraterrestrial manufactured artifact, then I will refund approximately *eight times the amount of the royalty paid to me for each book sold.*

In making this offer, without any urging by my publisher, I risk personal bankruptcy—the loss of everything I have managed to save in a lifetime. This does not prove that my appraisal is correct, but it does provide a meaningful measure of the extent of my confidence after nearly a decade of research in the UFO field.

A Rational Approach to the UFO Problem

T. Patrick Rardin

A rather cursory review of the history of beliefs held and defended by various people at various times may lead one to the conclusion that there is no limit to what humankind can believe. There is nothing inherently wrong with that conclusion. Problems do arise, however, when that conclusion is used to justify certain intolerant social, political, or religious constraints on the intellectual life of the individual. Certainly the constraints and the force exercised in their enforcement can cause far more damage to society and the individual than any belief alone, however offensive.

On the other hand, it would be just as absurd to argue that the fact that humankind has believed any number of bizarre things supports the view that beliefs are so personal and subjective that there can be no distinction between rational and irrational, or acceptable and unacceptable beliefs. As rational beings, we do make judgments about the acceptability and unacceptability of beliefs. Whether a particular belief is acceptable or unacceptable may depend on a number of factors, and may at times appear next to impossible to determine. But to conclude that there is no distinction to be made in such a case would be fallacious.

There have been a number of different beliefs held and argued in connection with the spate of reports of Unidentified Flying Objects, or UFOs, over the last forty years. There are those who believe that anyone who takes those reports seriously is a good candidate for psychotherapy. Others believe that we are in no position to judge the accuracy of those reports, or that everyone is entitled to his or her own belief.

But there is a middle ground. UFOs can be discussed seriously, intelligibly, and to some extent can be discussed without quibbles over particular "sightings." A first necessary step here is an attempt to clarify the subject under discussion. With that much done, we can get on with the task of evaluating certain beliefs concerning UFOs.

What Are UFOs?

The first step toward any serious and intelligent discussion of UFOs involves the adoption of a definition. According to Hynek, a UFO is: [1]

> the reported perception of an object or light seen in the sky or upon land the appearance, trajectory, and general dynamic and luminescent behavior of which do not suggest a logical, conventional explanation and which is not only mystifying to the original percipients but remains unidentified after close scrutiny of all available evidence by persons who are technically capable of making a common sense identification, if one is possible.

With this definition, Hynek believes he can make a distinction between misperceptions and misidentifications of known objects and events, weather balloons, normal aircraft, and so on, and those reports that deserve serious scientific investigation. And using this definition, Hynek goes on to discuss a number of problems presented by the occurrence of UFOs. One of these problems is whether UFOs, as genuinely new empirical observations, require new explanation schemes that would revolutionize extant scientific theory.

But let us take a moment to consider this definition and the consequences of accepting it. First of all, the definition does not make a distinction between evidence that is available in fact and that which is available in principle. This is no small matter. Temporal, financial, and practical limitations may result in the failure to perform certain tests which would provide us with additional evidence. That evidence may be crucial to the identification of the object or event as one with which we are presently familiar. Thus, if by "all available evidence" Hynek means all available evidence *in fact,* then his definition will include as UFOs quite normal objects which we do not have the means of firmly identifying as such at present.

Let us assume then that by "all available evidence" Hynek means all evidence available in principle. If we do not make that assumption, the problem Hynek sets out to solve—whether UFOs require new explanation schemes—may well cease to be a problem. The problem is of a theoretical kind that calls into question the ability of extant scientific theory to explain UFOs. But without the assumption mentioned above, there is the possibility that every UFO may be explained and identified at some future time as an object with which we are presently familiar under extant theory. With that possibility left open, the problem surrounding UFOs cannot legitimately be identified as a theoretical problem of the type characterized by Hynek. the problem may simply be of a practical kind that involves the establishment of conditions under which extant theory can be brought to bear on the question of the identification of UFOs.

Consider also a second difficulty with Hynek's definition. On that definition, whether UFOs constitute genuinely new empirical observations is

not itself an empirical matter. By definition, UFOs will constitute genuinely new empirical observations. For on Hynek's view, a genuinely new empirical observation is simply one that cannot be explained by extant scientific theory. That UFOs are genuinely new empirical observations follows straightforwardly from the definitions employed. The empirical question, then, is not whether UFOs would require new explanation schemes, but whether there *are* any objects that can be identified as UFOs given the definition he employs.

The last point is that it is not entirely clear that UFOs, even as genuinely new empirical observations, would require new explanation schemes that are, as Kuhn characterizes them, "not only incompatible but often actually incommensurable with that which has gone before."[2] Perhaps all we need to explain UFOs would be the addition of a few new, minor hypotheses to extant theory.

I will not here further consider, however, what theoretical changes UFOs would require. Instead, I shall argue that there is a certain class of hypotheses—the extraterrestrial hypotheses—that can be shown on quite simple, rational grounds to be unacceptable.[3]

The Acceptability of the Postulated UFO

Despite the objections presented above, let us accept at least the spirit of Hynek's definition of "UFO." Within the confines of his investigation in the book cited earlier, we find ourselves with at least seventy-six legitimate UFO sightings. These are distributed over a twenty-year period from August 1950 to August 1970, and average to about four reports per year. I will use that average as a basis for the following discussion.

One of the cases is reported to have occurred on October 26, 1958 at Loch Raven Dam, Maryland. Two observers reported a luminous object descending from the sky at a 45° angle to the height of some telephone wires. It moved across the farmyard and disappeared over a small silo. How are we to explain this sort of thing?

We might adopt the following hypothesis:

H_1: All UFO contacts are contacts with some form of extraterrestrial intelligence.

H_1 provides us with a simple and concise explanation of the event described above and others like it. If we remain fairly strict about what counts as a UFO, and if we accept Hynek's data in other accounts, we will make use of the hypothesis only about four times a year. All other cases can be written off as either misperceptions or misidentifications.

But even if we confine UFO occurrences to an average of one per year, there appear to be sound reasons to find H_1 unacceptable.

Carl Sagan has argued that we need only consider the consequences of

accepting H_1 in order to evaluate it.[4] To do so, we need a reasonable estimate of the number of advanced technical civilizations in our galaxy.

The basic structure of Sagan's argument is of the form traditionally known as Modus Tollens. Sagan begins by supposing that H_1 is acceptable. Given the number of UFOs to be explained and a reasonable estimate of the number of advanced technical civilizations in our galaxy, Sagan deduces the number of launches required of each such civilization each year. In effect, Sagan shows that if H_1 is acceptable then a certain number of launches per year per civilization must also be acceptable. But the crux of Sagan's argument is that since the number of launches required to explain the assumed number of UFO sightings is unacceptable, then H_1 is itself unacceptable.

There are seven factors involved[5] in the calculation of N:

(i) The mean rate of star formation in the galaxy, R_*. Since the number of known stars in our galaxy is approximately 10^{11} and the age of our galaxy is approximately 10^{10} years, the value of R_* is set at approximately 10.

(ii) The fraction of stars in our galaxy with planetary systems, f_p. f_p is estimated on the basis of the frequency of what are referred to as dark companions of nearby stars, stellar rotation, and contemporary theories concerning the origin of our solar system. The occurrence of planets about stars is not an extraordinary event. And so f_p is set at approximately 1.

(iii) The mean number of planets in planetary systems that possess environments favorable to the development of life, n_e. The value estimated here follows from considerations of our own solar system and projections to main-sequence stars of the spectral type F2 through K5. "Main sequence" refers to the period of stellar evolution during which a star's temperature and luminosity remain constant over a rather long period of time. Spectral typing is based on information derived from a spectroscopic reading that determines a star's color index. The color index is the difference between the photographic index and the apparent visual index, which are based on the detection of blue and yellow light, respectively. Spectroscopic typing is the basis for stellar classification, and the initial class distribution is designated by the following letters:

O B A F G K M

By the same procedure, each initial class is divided into ten subgroups of stars. On the basis of spectroscopic readings also used to determine the mass, radius, chemical composition, energy radiation, and temperatures of the surface layers of stars, stars of the spectral types between F2 and K5 are considered suitable for the development of life. The value of n_e is accordingly estimated at approximately 1. This means that there is at least one planet for each of the main-sequence stars of the types between F2 and K5 that is suitable for life.

(iv) The fraction of favorable planets on which life does develop, f_j. The value estimated for f_j is largely dependent on investigations of our own situation here on earth. Its value is estimated at approximately 1.

(v) The fraction of planets on which intelligent life with manipulative abilities develops within the lifetime of our local star, f_i. The limiting requirement is that of manipulative abilities. f_i is estimated at approximately 10^{-1}. In other words, ten percent of all main-sequence stars between the spectral types F2 and

K5 are estimated to have a planet within their system on which intelligent life with manipulative abilities developed.

(vi) The fraction of planets on which intelligent beings have advanced to a technological level approximately 200 years beyond our own, f_c. Such civilizations are generally referred to as advanced technical civilizations. The estimated value for f_c is based exclusively on our history. It involves sociological, political, and anthropological considerations, to mention only a few. The value estimated for f_c is approximately 10^{-1}.

(vii) The lifetime of an advanced technical civilization, L. If we consider our own state of development, we find that we are in a critical period. We have the ability to destroy virtually all forms of life on our planet. Since there are those who have recently held high political offices and believed that certain situations would justify that catastrophe, it is not so clear that we have made up our minds not to do so. Projecting this situation onto the galaxy, we can consider two alternatives with respect to the value of L. If the civilization destroys itself early in its development, L may be estimated at approximately 10^2 years. If the civilization passes its critical period, it is unlikely that it will then destroy itself. In this case, L may be estimated as greater than 10^8 years. These two values for L result in conflicting values for N. Sagan estimates the mean value to be approximately 10^7 years.

If we substitute the values above into the following formula:

$$N = R_* f_p n_e f_j f_i f_c L$$

we get as an estimated value for N:

$$N = \text{approximately } 10^6$$

In other words, there are approximately 10^6 advanced technical civilizations in our galaxy.

In order to estimate how many space launches each such civilization would undertake each year, we must consider the number of places in the galaxy that might be of interest to the 10^6 advanced technical civilizations. Anyone who accepts H_1 must believe that earth is one of them. It must be remembered that our civilization is not included in the value of N. So if earth is a place of interest, then we can assume that each of the 10^6 advanced technical civilizations is of interest as well. It would also appear that all planets on which intelligent life with manipulative abilities has evolved are equally interesting. The advanced technical civilizations may even be interested in solar systems in which intelligent life has not evolved; we should not be so self-centered as to think intelligence is the only thing of interest to the intelligent. Following Sagan, it seems safe to assume that there are approximately 10^{10} interesting places to investigate in our galaxy.

Now if only one UFO sighting per year is a sighting of some form of extraterrestrial intelligence and if earth is not unique in this respect, then the

mean launch rate per civilization per year is determined by the following equation:

$$10^6Q = 10^{10}$$

Each advanced technical civilization must launch an average of 10^4—ten thousand—spaceships per year. This is about twenty-seven per day, or more than one per hour. This result, according to Sagan, is unacceptable. Therefore, H_1 is unacceptable as an explanation of UFOs.

It is clear from the above formula that if the acceptance of H_1 as an explanation of one UFO sighting commits us to ten thousand launches, then its acceptance as an explanation of four UFO sightings commits us to forty thousand, and so on. H_1 is unacceptable as an explanation of the cases of UFO sightings that Hynek believes are in need of explanation.

The argument just considered is designed to show that H_1 is an unacceptable hypothesis. How good is this argument? On the one hand, it is surely valid. It is a simple application of Modus Tollens, and if its premises are all true then the truth of the conclusion logically follows. But on the other hand, the soundness of the argument rests on a number of factors. The most important factor is Sagan's claim that ten thousand launches per year per civilization is unacceptable; it is used as a critical step in the deduction of H_1's unacceptability. *Is* this an unacceptable number of launches? Without some support or standard by which we can determine that ten thousand launches is unacceptable, we have yet to show that H_1 is itself unacceptable.

In order to avoid a rather sticky question of how many is too many, we need another way of evaluating H_1. Fortunately, there is such a way. We can evaluate the acceptability of H_1 by evaluating the acceptability of another extraterrestrial hypothesis on which the acceptability of H_1 is dependent. If we can show that this considerably weaker hypothesis is unacceptable, then we will have shown that H_1 is unacceptable as well. In the following section, I construct the weakest form of the extraterrestrial hypothesis on which the acceptability of H_1 is dependent. I then argue that the weaker form of the extraterrestrial hypothesis is unacceptable on grounds of rational choice.

The Price of Postulating UFOs

Let us consider the weakest hypothesis we might propose in order to establish a link between UFO sightings and some form of extraterrestrial intelligence. That hypothesis can be stated as follows:

H_2: There is at least one UFO contact that is a contact with some form of extraterrestrial intelligence.

H_2 alone, in contrast to H_1, cannot be said to explain more than one UFO sighting. H_1 purports to explain as many UFOs as we can identify as such.

But the acceptance of H_1 along with other reasonable assumptions—that earth is of no special interest, and that there are approximately 10^6 advanced technical civilizations in our galaxy—was shown to have a rather odd consequence. Accepting H_1 as an explanation of only one UFO sighting committed us to a rather high number of launches. In this regard, H_2 appears to have an advantage over H_1. For although H_2 alone may explain only one UFO sighting, its acceptance does not commit us to more than one advanced technical civilization.

But if we are willing to accept 10^6 as a reasonable estimate of the number of advanced technical civilizations in our galaxy, then H_2 may be employed in a rather complicated argument that does purport to explain more than one UFO sighting—in fact, as many as you care to identify as such. This argument does not depend on any "earth has special status" premise, nor does it require that we abandon our commitment to only one launch per civilization. Consider the following argument.

If we accept H_2, then we have committed ourselves to one advanced technical civilization and one launch to earth. Intuitively, those consequences do not appear absurd. Since H_2 alone does not commit us to an absurdity, we can accept it.

But here we need not restrict ourselves to the bare assumption that an advanced technical civilization exists. We can also say something about it. If such a civilization embarks on an expedition in search of intelligent life, it can be expected to do so systematically. Sagan's assumption of individual launches to individual places seems a bit absurd in this regard. Why not suppose the establishment of a "home base" relatively close to a number of solar systems worthy of investigation, from which shorter trips to various points of interest are made? Our solar system, we might suppose, is at least in the reach of one such "home base."

At this stage in the argument, given the number of places in which that civilization may have evolved and from which it begins its search for intelligent life, the possibility of earth being contacted within a reasonable period still appears rather remote. But here we can close the gap between our assumed advanced technical civilization and its relatively close home base by assuming as before that there are to be 10^6 such civilizations distributed throughout our galaxy. This will increase the number of launches required in the galaxy at large as well, but no more than one for each such civilization and one toward earth. On the basis of that estimate, Sabastian von Hoerner estimates the average distance between advanced technical civilizations at approximately 10^3 light years.[6] Since the average lifetime of such a civilization is estimated at approximately 2.4×10^4 years and we are only a few hundred years from that state of development, we can use 10^3 light years as the approximate distance between our solar system and that of an advanced technical civilization.

Furthermore, since we are talking about an *advanced* technical civilization, we can assume that they have solved a number of problems involved in interstellar travel. Therefore, we assume that they can travel at an average of ten light years every ten earth years. If the home base mentioned earlier is approximately ten light years from our sun, in the vicinity of Epsilon Eridani, it would require a trip of less than one thousand years to get there. From their home base, it would take another ten years to arrive at earth.

We might also suggest that our solar system will be one of the home bases in the search for intelligent life. The chance of that actually taking place is estimated to be once every 10^5 years.[7] This too follows from our assumption concerning the number of advanced technical civilizations in our galaxy. And that assumption does not conflict with there being only one such launch toward earth.

On the basis of these assumptions, it might be concluded that the recent UFO sightings from 1947 to the present are sightings of one spaceship originating from an advanced technical civilization on a random search for intelligent life. Once on Epsilon Eridani, say, they left for earth in 1937. Since 1947, they have been traveling about earth gathering samples for investigation.

As folksy as this explanation is, the multiplicity of UFO sightings, regardless of the strictness of our definition, are brought under an assumption of only one initial flight to earth. The number of different objects reported at any given time as well as the various shapes reported at different times can be written off as misperception and misidentification, or we may simply increase the number of ships of various shapes, a flotilla, launched at one time. And this (admittedly complex) explanation seems to avoid the numerical difficulty of H_1. We can account for numerous legitimate sightings without committing ourselves to an "unacceptable" number of initial launches.

In order to evaluate this explanation and the initial hypothesis H_2, let us review some of the assumptions involved: (a) the extraterrestrial spacecraft landed on a planet of Epsilon Eridani or on a planet of some relatively close solar system prior to its arrival on earth; (b) the advanced technical civilization has achieved a rate of travel of ten light years every ten earth years; and (c) the extraterrestrials would actually make the trip to earth, if they had reached a relatively close solar system.

The first assumption is certainly reasonable. It is essentially that an advanced technical civilization has evolved in a solar system that is relatively close to ours. Given extant theory, the closest such solar system is Epsilon Eridani. It is of the spectral type K2 and is a main-sequence star. The spectral range of habitable solar systems is from F2 to K5.[8] But we need not hold dogmatically that Epsilon Eridani has produced an advanced technical civilization, nor that Epsilon Eridani will be visited first. All that is assumed is that there will be *some* such solar system investigated prior to earth.

The second assumption is also reasonable, if we ignore practical limitations on spaceflight. The theoretical principles involved fall under the domain of relativistic mechanics, and here the velocity and energy required set the limits.

The trip could be made at the required time if a top speed of .99c, where c is the speed of light, is obtained halfway through the flight.[9] It accelerates up to that point, and it decelerates after that point until it reaches its destination. The mode of propulsion that is *most efficient* would require the complete annihiliation of the mass of the propulsion material. This involves the use of a matter–antimatter propellant which unlike other fusion reactions in nature, for example, burning hydrogen to helium, results in the entire mass of the propellant being converted to energy.

The third assumption, however, is unacceptable. And if that background assumption is rejected, then H_2 must be abandoned. And if H_2 is unacceptable, then certainly H_1 is too. For if not even one UFO sighting can be reasonably explained as contact with some form of extraterrestrial intelligence, then no UFO sighting can be so explained.

To understand why the third assumption is unacceptable, we need only consider the situation as one involving rational choice. In such a situation we suppose an individual is presented with a number of alternative courses of action from which one course of action is to be chosen. Our assumption of rationality is that the individual will choose in his or her best interest.

For purposes of discussion, let us again make some assumptions. We suppose our extraterrestrial travelers arrive at Epsilon Eridani (or some similar place) before they arrive at earth. We also assume that their goal is simply initial contact. Now if their first arrival on earth was in fact in 1947, then they would have left Epsilon Eridani in 1937. But by that time, there would have been a sufficient amount of radio waves escaping from earth to detect intelligent life on earth. Radio waves, of course, travel at the speed of light.

We compare, then, the cost in terms of power between direct contact by actual travel and indirect contact via radio waves. Assuming the *most efficient* mode of travel, the total annihilation of mass, the power–mass relation in terms of watts required for one gram is given by the following equation: [10]

$$P = 3 \times 10^6 \text{ watts/g}$$

So if a spacecraft weighed only one gram, a power output of 3×10^6 watts would be required to achieve the desired speed, .99c. Under the same circumstances, an initial contact by means of radio waves would cost 10^2 watts.[11] Were we to find ourselves in the circumstances outlined in this situation, it would certainly not be in *our* best interest to choose direct contact over radio contact. In general, such a choice is not in the best interest of any

individual regardless of his or her point of origin. For that reason, H_2 is unacceptable.

Thus if we were to adopt H_2, we would then have to conclude that some group of extraterrestrial beings are not acting in their best interest. But that clearly conflicts with the notion of an advanced technical civilization. At least part of the meaning of "advanced" entails the ability to solve a rather simple problem involving rational choice.

Conclusion

My attempt has been to consider rational grounds for the acceptance or rejection of extraterrestrial hypotheses as explanations of UFOs. H_2 is the weakest form of the extraterrestrial hypothesis, and H_1's acceptability is dependent on H_2's acceptability. It follows, then, that any basis for the rejection of H_2 is a basis for the rejection of H_1. And H_2, as we have seen, is unacceptable: its acceptance would require that we accept that the choice of earth on the part of the "advanced" civilization is irrational.

My argument against the acceptability of H_2 does not depend on the assumption that Epsilon Eridani is the point of departure for an earth expedition. The relative costs involved hold constant regardless of the distance.

My argument does depend on the assumption that the extraterrestrials have knowledge of earth's intelligent form of life. Otherwise, radio or whatever other kind of indirect contact would not be an option. But that assumption is harmless. If they had no knowledge of earth's intelligent form of life, they would have virtually nothing to gain by actually making the trip. Our solar system is not unique. On the average, there are far too many solar systems without intelligent life in the "immediate" vicinity of any postulated advanced technical civilization to warrant the energetic expense required to travel to earth. For we must remember that on the most optimistic estimates concerning the number of advanced technical civilizations in our galaxy the average distance between them is 10^3 light years.

We must also bear in mind that the expected utility of contact decreases proportionately as the effort required to make contact increases. The effort required to conduct an undetected exploration for the purpose of an in-depth exploration of earth in particular would render the expected utility of that course of action considerably less than that of indirect contact. For all that the extraterrestrials could hope to gain by an undetected exploration would be that which they could obtain either by monitoring our output from a relatively close home base or by investigating a solar system without intelligent life.

In closing, I must point out that there is always the *possibility* that H_2 is true. An advanced technical civilization may on occasion make mistakes and not choose in their best interest. Extraterrestrials *may* even be living among

us. But before we mount a campaign to identify them we had better substantiate our claims. There is at present no evidence linking UFOs to extraterrestrial intelligence. But that fact does not preclude an intelligent discussion of the claims that purport to establish such a link. As things stand, there are good reasons for rejecting them.

Notes

1. J. Allen Hynek, *The UFO Experience: A Scientific Inquiry* (Chicago: Henry Regnery, 1972), p. 10.

2. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, International Encyclopedia of Unified Science, vol. 2, no. 2 (Chicago: University of Chicago Press, 1970), p. 103.

3. The arguments employed are for the most part based on presently accepted scientific theory. Anyone who is in favor of rejecting the established framework of science because it necessarily excludes UFOs from the realm of the explainable may stop reading at this point.

4. Carl Sagan, *The Cosmic Connection* (New York: Doubleday, 1973), p. 200ff.

5. Most of the work in this area is a result of a conference on intelligent extraterrestrial life sponsored by the Space Science Board of the National Academy of Sciences. See Carl Sagan, ed., *Communication with Extraterrestrial Life* (Cambridge, Mass.: MIT Press, 1973). Also see I.S. Shklovskii and C. Sagan, *Intelligent Life in the Universe* (San Francisco: Holden-Day, 1966), ch. 29.

6. Sabastian von Hoerner, "The Search for Signals from Other Civilizations," *Science* 134 (1961): 1839.

7. Shklovskii and Sagan, p. 450ff.

8. Ibid. p. 342ff.

9. See Edward Purcell, "Radioastronomy and Communication through Space," USAEC Report BNL-685; and Sabastian von Hoerner, "The General Limits of Space Travel," *Science* 137 (1962): 18.

10. For the derivation of the formula see von Hoerner, "General Limits."

11. There are problems involved in establishing radio contact. The extraterrestrials must suppose that we could interpret their signal to insure contact. But this problem and many others discussed in the literature are of a practical nature. If we did not eliminate practical constraints, the whole discussion would be pointless. Interstellar travel is practically impossible.

Science, Proof, and the Ancient Astronaut Hypothesis

PASQUAL S. SCHIEVELLA

That extraterrestrial intelligences visited earth in antiquity and altered the course of human history is an old hypothesis, offered anew by Erich von Däniken. Although I am sympathetic to this thesis, the main thrust of this paper is a criticism of the behavior of critics, particularly scientists, in the broadest sense of that term. As a historical hypothesis, the merits of the ancient astronaut hypothesis should be weighed against the available evidence with rigorous scientific scrutiny.

Unfortunately, this has not occurred. Instead the scientific community has attacked both the hypothesis and von Däniken himself with outrage and abuse. His evidence has been dismissed with ridicule, and the hypothesis assailed with fallacious reasoning and an appeal to authority. Equally disturbing is the stony silence of "experts" regarding the evidence for the hypothesis. This silence, and the attacks of critics like Ronald Story,[1] supported by Carl Sagan, clearly show that a scientific evaluation is not forthcoming.

In what follows, I shall attempt to present the case for von Däniken. I shall then examine some of the criticisms frequently made against the ancient astronaut hypothesis.

It should be noted at the outset that this examination of such irrationality on the part of many scientists is in no way to be interpreted as a lack of faith in science. In point of fact, I have as much faith in science as some believers have in the existence of an anthropomorphic God. Religious faith, however, has evolved out of irrational hopes, fears, and reification of the unknown. It is a faith for which there is no evidence, in any reasonable sense of that term. It is indeed, more often than not, a faith in spite of evidence to the contrary. *My* faith in science, on the other hand, has evolved from the achievements of science, from its rational and self-corrective method, and from the public evidence it has offered, however incomplete, in fulfillment of its predictions.

It must be understood, then, that I am an ardent proponent of science. That is why I become so disturbed by dogmatists like Carl Sagan who destroy the credibility of science by claiming for it more than it can deliver and by refusing to give the ancient astronaut hypothesis the scientific consideration it deserves.

The Case for von Däniken

It is common knowledge in our scientific era that it is both possible and probable that other intelligences exist elsewhere in the universe. To assume otherwise is to regress to the Middle Ages, when it was believed that the earth was the center of the universe and man the supreme creation.

Historian Will Durant, in his *Story of Civilization*, suggests that we are not necessarily the descendants of the primitive cultures to which archaeologists and anthropologists like to attribute our ancestry. His thesis, and the mysteries that science has not explained, strongly suggest the possibility that ancient space travelers visited earth. No argument based on such data as problems of intergalactic travel and the vastness of space has yet proved that superior intelligence could not accomplish what we, with our few centuries of limited scientific technology and theory, believe to be impossible.

It is both possible and probable that ancient astronauts did visit earth. This cannot be denied unless one holds that evolution is impossible, or that there is no evolution and God created only us (a point that raises questions on which *no* evidence could be brought to bear), or that such evolution as there has been took place only on earth, or that except for us there are no astronauts or other intelligences in the universe, or that the evidence is all in as to our origin, or that we have *absolute* knowledge about these things, and the like. Surely no enlightened person could hold such medieval ideas.

Unless we deny the possibility of evolution elsewhere in the universe or pretend to an absolute knowledge regarding our past, we must recognize at least the *possibility* that technologically advanced civilizations may have arisen elsewhere and that they may have visited us in the remote past.

The ancient astronaut hypothesis, then, is at least possible. As to proof of von Däniken's theories, it must be noted that the ancient astronaut hypothesis is not a matter of natural science. It cannot be expected to follow the rigid rules and standards of proof set for natural science. Instead, its modes of proof are primarily like those in the social sciences, such as psychology, sociology, and anthropology. To expect formal rigidity in such informal disciplines is to demand what cannot be. Nevertheless, at the very least, one would expect scientists to permit von Däniken to extrapolate from his data, since they themselves accept extrapolation as one kind of evidence permitting further advances in science.

What *could* constitute proof for the ancient astronaut hypothesis? We are not likely to find an ancient astronaut. As von Däniken points out, "crashed"

spaceships from the distant past would probably long ago have disintegrated or possibly have been carried away piecemeal. What then?

Von Däniken's thesis explains many hitherto inexplicable mysteries none of which has received any elucidation from academic minds fettered by prejudices and preconceptions. It is not fatal to the hypothesis that critics find errors or misinterpretations. Taken as a whole, von Däniken's findings point convincingly to the likelihood of extraterrestrial interference in man's distant past. That is not to deny that von Däniken manipulates many of his facts to adapt them to the ancient astronaut hypothesis. But what scientist does not do this when he formulates a theory?

The ancient astronaut hypothesis is little different from most of recorded history. The hypothesis requires only "validation" of the reported data through correlation of those data with the unexplained and wondrous technical artifacts of the distant past. The proofs of the ancient astronaut hypothesis are to be found in the logic of both possible and probable events, in the historical (even though predominantly religious) documents that are held in such high historical esteem throughout the world, and in the ancient artifacts that cannot be explained in terms of the supposed human knowledge and capabilities of antiquity. All these, studied as a body of coherently describable data, point to the likelihood of extraterrestrial intervention. Furthermore, the descriptions in ancient documents, when *coupled* with empirical data, considerably weaken the argument that *terrestrials* are responsible for those artifacts, which obviously were beyond their linguistic, conceptual, and technical abilities. Let us consider some of those wonders. A few should suffice.

At the Bay of Pisco, south of Lima, Peru, there is an enormous trident engraved on the side of a hill pointing directly at what is known as the Nazca Plains, outside the small city of Nazca. There one can find what are now called the Nazca ground drawings. Inscribed on approximately thirty square miles of arid Nazca pampas are huge drawings of a spider, a monkey, a hummingbird, and the like. They are so large that they can be recognized only from the air. Other drawings could easily be mistaken for aircraft landing strips. Some are merely straight (often parallel) lines running across rough terrain and up mountainsides, without deviating a fraction of an inch—sometimes almost ten kilometers (6.25 miles) long, as if cut by a laser beam from on high. As to their source and meaning, there are no accepted explanations. A NASA engineer, Robert Earle, claims to have determined that most of the lines point to important geographic locations on the earth.[2]

Another unexplained mystery is that of the Terraces of Baalbek in Lebanon, where huge stone blocks sixty feet long and said to weigh 2,000 tons have been moved into place. They are so massive that even our modern technology could not handle them.

Then there are the so-called "fortress" walls at Sacsayhuaman, outside the city of Cusco in Peru. There are thousands of enormous stones, many tons in weight, fitting together as closely and as neatly as the pices of a jigsaw puzzle, without any kind of connecting adhesive. The thin edge of a sheet of paper could not be inserted between them.

Another marvel is the recessed quadrangular wall at Tiahuanacu, outside of La Paz, Bolivia. The inside surface is studded with sculptured faces apparently representing every racial type on earth. There are many hundreds of other unexplained mysteries which most scientists show no inclination to investigate. I shall mention only one more: the mystery of the existence of models of sophisticated aircraft. Some of these models show a separation space indicating the possible existence of nuclear engines. Such models, which are in museums throughout the world, have been tested and found to be aerodynamically accurate in design.[3] They are amazingly interesting artifacts because they correlate so well with the many scriptural descriptions of flying machines emitting smoke, fire, and thunderous noise.

Even if we accept the claim that all these things originated with terrestrial beings, we would be hard pressed to explain the disappearance of such superior civilizations. We have found no documentary evidence or, indeed, evidence of any kind to support a terrestrial origin for such technological achievements.

It seems, then, as von Däniken reiterates, that it is time to bring to bear on these fascinating mysteries, and on their description in the languages of antiquity, new perspectives and viable hypotheses made possible by the more sophisticated language and knowledge of our day.

If scientific and religious institutions would allow it, and if governments or foundations would advance funds to support it, researchers could feed data from all over the world into computers to determine the comparative similarities among empirical descriptions of "gods from space" and to determine whether these descriptions are, as the critics prefer to believe, nothing more than the creations of insane minds or over-fertile imaginations. Supplemented by the computers, experts in comparative linguistics, translation, ancient cultures, and ancient languages should be able to determine whether the technical data, concepts, and achievements found in museums, existing at archaeological sites, and described in historical and religious documents could have originated with a pre-scientific people who spoke only non-technical and unsophisticated language.

As it stands now, the ancient astronaut hypothesis is primarily a historical hypothesis and peripherally a scientific one. It is founded on documentary and circumstantial evidence and, in some cases, on hard evidence that may not be denied except by stretching the facts beyond reason and probability.

Von Däniken's Critics

Many professionals are sympathetic to von Däniken's concerns. Among the millions who read his books are scientists, philosophers, archaeologists, anthropologists, engineers, and scholars from every field. This is what apparently frightens scientists like Sagan, who seems to be making every effort to silence scholars who are inclined to take the ancient astronaut hypothesis seriously. He even goes so far as to suggest that if we do not agree with him, we are not reasoning persons. Such attacks are apparently calculated to intimidate the reader, particularly if he is a scholar. With such a modus operandi, it is not at all surprising to see that the critics of the ancient astronaut hypothesis find support from representatives of the church, an alliance that would surely amuse Galileo.

Much of the negative criticism of Erich von Däniken and the ancient astronaut hypothesis emanates from a relatively small number of scientists led, it seems, by Carl Sagan, who claims that there is not a "smidgen of evidence" for the hypothesis. The claim is that they *prove* their theories, whereas the ancient astronaut theorists do not. I wish to remind Sagan and his followers that there are many kinds of proof. Sagan knows, but he appears to pretend otherwise, that proof can be not only empirical, observational, experimental, or inductive (as he seems to imply), but also theoretical, logical, mathematical, hypothetical, deductive, statistical, probable, and documentary. These various forms account for much critical and fundamental examination with the sciences. The ancient astronaut theorists' use of many of these kinds of proof is no less valid than the sciences' use of them. Here I must insist that evidence, proof, and knowledge must be possible, probable, public, and predictable. They must be directly or indirectly verifiable and compatible with a comprehensive body of relevant facts, theories, generalizations, and hypotheses. To the degree to which these elements fit coherently without contradiction, to that degree we can make legitimate claims to having evidence, proof, or knowledge.

A deliberate ignoring of the different kinds of proof is the modus operandi of those scientists who are guilty of dogmatism and prejudice. Are von Däniken's scientific critics suggesting that there is no truth or knowledge except for that found in the natural sciences? Would Sagan et al. claim that there is not a "smidgen of evidence" that Lincoln was shot at Ford Theater, or that the history of the United States occurred substantially as recorded, on the ground that no one alive was there to see it happen? Would they question the documents that record Fleming's discovery of penicillin, or Caesar's having ruled Rome? Such knowledge cannot be verified by the techniques of the natural sciences, by experimentation, or by unfalsifiable data.

Of course science must conduct its search for truth objectively. But *science* is not *scientists*. The latter are far from infallible and often far from objective; a few are even dishonest, prone to authoritarianism and dreams of scientific infallibility even when they admit they are speculating. They forget that the present achievements of science constitute little more than an embryonic development in the continuing search for truth. In elevating themselves to the throne of infallibility, they imply that they alone have the key to the mysteries of the universe. They display the worst kind of parochialism in failing to see that there are problems of a kind that cannot be solved by a study of subatomic constructs or of molecular structures. To make matters worse, the technical achievements of science are often conflated with scientific certainty, misleading the layman into believing that the products of technology–such as television, nuclear weaponry, and space travel–are ample proof that what scientists say is ipso facto true.

Although von Däniken is no stranger to scientific concepts in this field, he has never pretended to be a scientist. His critics ignore this, however, and argue against their own assumption that von Däniken thinks he is a scientist. They then proceed to show what he himself admits–that he is not. This straw-man approach is typical of much of their criticism. Indeed, it is probably to our benefit that von Däniken is not a scientist. As someone has said, "Science is too important to leave only to scientists." The ideas von Däniken expresses have been posed, as von Däniken says, thousands of times in the past. No one before, however, was sufficiently daring or provocative to raise to world-wide awareness the possibility and even probability of extraterrestrial intelligence.

Various critics have referred to von Däniken's research as "pseudoscience," and claim that it belongs with occult literature. This can hardly be called scientific disproof of the ancient astronaut hypothesis. It sounds more like the voice of embattled authority proclaiming "truth" loudly and anxiously. Can such a response be attributed to von Däniken's critics' annoyance with his provocative style? It is, after all, very strange that they have not attacked with the same concentration and intensity any of the hundreds of other ancient astronaut theorists–enthusiasts from Homer to Landsbury to Josef Blumrich.[4] Is it because these others have not exposed the unwillingness of scientists to concern themselves with the ancient records buried in the world's archives, waiting to be deciphered? Is it because von Däniken is shaming scientists and other authorities for their lack of integrity? Is it because they do not like having their self-limited search for truth exposed? Is it because they wish to ignore certain facts that abound throughout the world? Is it because they are afraid that the generous research appropriations they receive might be diverted to this cause?

Indeed, von Däniken's persistence and his provocative and accusatory style of writing have not endeared him to the world's scientists, nor to its

religious leaders. However, if his questions are not proper, it should be easy for those with knowledge to demonstrate that fact. Scientists certainly have not shown von Däniken's characterization of them to be wrong, and what is worse, their unscientific responses support his contentions. Nor have devastating proofs against his hypothesis been demonstrated. Certainly character assassination and weak claims will not do.

The attacks and negative responses from religious institutions (and individuals like the contributors to *Some Trust in Chariots*, partly edited by the Reverend B. B. Thiering, chaplain of Cranbrook School) are easily understandable, in view of their special interest. One would expect, however, a more sympathetic attitude from the Sagans of the scientific community. Their approach thus far smacks entirely of dogmatic authoritarianism. Obviously science cannot be expected to preoccupy itself with claims that can be shown to be filled with internal contradictions, ambiguities, and meaningless terms. But such is not the case with von Däniken's thesis.

Consider, as a typical attack on von Däniken, Story's *The Space-Gods Revealed*. In his foreword, Carl Sagan suggests that *Chariots of the Gods?* is "pop religion." It is in fact an *attack* on religious claims. Moreover, Sagan sets the tone for disbelief before Story presents his so-called evidence. He makes it clear that—in his view—von Däniken is trampling where he does not belong. As Sagan has said on his "Cosmos" program, putting the cart before the horse, the amateurs scare away the professionals. But that is no more than a lame excuse for their failing to deal with the problem, certainly not an explanation. In fact, amateurs tread where the professionals refuse to go. *Someone* has to investigate unsolved mysteries. Further still, without discussing the extremely complex nature of the term "evidence," Sagan dogmatically states that there is no evidence for ancient astronauts. Both Story and Sagan put words into von Däniken's mouth—words like "dummies" in reference to our ancestors. Von Däniken does not claim that our human ancestors were "stupid," or that they were not genetically similar. He merely suggests that they could not have had the knowledge implied by some of the artifacts from the past, however they may have received the genes we have inherited. He is quite correct. They lacked a sophisticated technical language, concepts, and insights needed to solve such problems. Story and Sagan invariably associate von Däniken with strange ideas and mystic cults, branding him guilty by association.

Following Sagan's foreword, Story's book is permeated with carefully chosen emotional words and ad hominem attacks calculated to ridicule von Däniken's ideas; he resorts to laughter and derision when logic and conclusive evidence are lacking. His handling of von Däniken and his theories is a paradigm of sloppiness cleverly concealed from those who lack logical training. Story confines himself primarily to von Däniken's earlier writings. Von Däniken was inexperienced with literature in matters of scholarly

research (he did not even graduate from high school). He felt he would be better received if he colored his writings with a fictional style. Thus these works are vulnerable to attack. However, Story cleverly avoids criticizing hundreds of facts and data from other of von Däniken's books. Here he would have been unable to draw on present-day accepted explanations, and on authorities with their pet theories to protect. Von Däniken insists we must marshal new interpretations based on present-day language, knowledge, and concepts.

The clearest indication of Story's philosophical and critical incompetence is seen in his treatment of von Däniken's explanations for the Nazca ground drawings. He says sarcastically, "The runways are not needed for space flight. What were the ancient astronauts flying—World War I biplanes?" Has Story not heard of our space shuttle, which requires a runway nearly three miles long? He implies that long landing strips would not have been needed. And, indeed, they may not have been. Nevertheless his remark assumes a great number of facts he could know nothing about, such as landing requirements, speed capacities, and so on. By suggesting that astronauts could have landed only in daylight, moreover, he is guilty of arriving at solutions in terms of our limited knowledge, precisely as von Däniken has charged. The extraterrestrials might, among other possibilities, have been sensitive to a far wider band on the spectrum than are we, enabling them to see at night, or they may have had instruments to amplify other forms of light that appear as darkness to us. Story also refers to the "soft, sandy soil of the Nazca pampa." But age, weather, heat expansion, and so on could have destroyed the original soil surface, and again he is assuming that their "vehicles" would be like ours, having similar landing gear. Finally, Story repeatedly appeals to the very authorities whose theories von Däniken places in question.

Story is unimaginative, and too comfortable with twentieth-century concepts to consider that the unthinkable may be true, as it so often has been in the past. His speculations will seem reasonable to those conditioned to think within present-day parameters. But that is precisely what von Däniken brings into question. It is scarcely necessary here to rehearse the list of great pioneers and discoverers, like Columbus, Galileo, and Pasteur, who were mocked and hounded during their lifetime primarily because their insights and theories ran counter to the "accepted" wisdom of the establishment of their day.

Have today's pundits learned nothing from their predecessors whose arrogance they unconsciously mimic? They speak knowingly, however mistakenly: "knowing" that theories are facts; and "knowing" that there is no real, hard proof for the ancient astronaut hypothesis. From a philosophical and even a scientific standpoint, there is no such thing as absolute proof—not even in the most seemingly solid sciences. By "real" or "hard" proof, Sagan actually refers to unscientific proof, for he seems to mean proof that is immediately accessible to our sense faculties. This is exactly the kind of proof

that once led the world to believe in a solar system with the earth at its center and the sun visibly revolving around it.

Story accuses von Däniken of not being aware "of the archaeological and historical facts." He will not accept that von Däniken *is* aware, and rejects them, refusing to accept hypotheses as facts. Yet Story himself claims to know the facts. He credits our ancestors with knowledge and common sense —even with simple techniques and equipment for moving objects weighing many tons—some up to eighty tons. He accepts their "empirical" descriptions of how they are supposed to have moved the huge statues (Moai) on Easter Island. He would deny that our ancestors' descriptions of ships in the sky with flaming tails, accompanied by thunderous noise and tremors of the earth, are empirical descriptions. He does not explain his criteria for choosing one description and rejecting another. But more than that, he seems to suggest that there are no ancient mysteries—mysteries that the ancient astronaut hypothesis might explain.

In discussing the date of an ancient artifact, Story attempts to cast doubt on the accuracy of von Däniken's information by use of the very subtle technique of leaving out crucial terms from von Däniken's comments, such as substituting "archaeologists" for von Däniken's "some archaeologists."

Although there are endless examples of distortions, innuendos, and illogic expressed by von Däniken's critics, we cannot dwell on them longer.

Conclusion

Von Däniken's critics seem to insist on one standard for him, that is, the empirical and positivistic methods, whereas they themselves rely heavily on theoretical or other methods. But they do not attack creationism in the Book of Genesis or the Christian belief in a non-material God. In this respect, we would do well to listen to P. E. Pothier of Bethesda, Maryland. In answer to an article that included attacks on von Däniken by Kendrick Frazier, published in *Science News* of 9 June 1976, Pothier said:

> May I remind those who deplore the increasing interest in the occult, as evidenced by the proliferation of occult books, that the number one all-time best seller remains the *Holy Bible*, the Manifesto of those bastions of occultism, the established churches. As I pointed out last fall, if . . . one must persist in this silly witch hunt, let him take on an enemy big enough to pose a serious threat to science and society. In the name of its peculiar form of occultism the Christian Church set up the Holy Inquisition to silence all heretics including scientists. It burned their books as well. It forbade and forbids all practical means of population control. In California it even forbids the teaching of evolution as fact. The "parascience cults" have done none of these things.
>
> If scientists really want to banish irrationalism from the world, let them hurl their challenge at the citadel of occultism—the Church.

If science sees its moral character and scientific ethic as including the responsibility to warn the world of what is and is not rational, then it has a responsibility to do so with a non-discriminating approach. If it attacks von Däniken, it has a responsibility to attack Reverend Moon, Billy Graham, Oral Roberts, and all the high priests of irrationalism—religious, political, and scientific, across the world. I have yet to hear any scientist attack religion because one of its popes said, "Faith is the highest form of reason." Have they lost the courage they manifest against Erich von Däniken, who unlike the satraps of religion is powerless to direct the wrath of the world on their heads? As a humanistic community, science should attack ideas, not proponents of ideas. It should resort to the scientific method, not to the tyrannical voice of dogma and authoritarianism.

Scientists would do well, therefore, to admit that ancient astronaut theorists have a right to use the same kinds of proof science uses in both the "hard" and "soft" sciences. They would do well to examine the evidence and data with the objectivity they claim to use in the "hard" sciences. Even Dr. Truzzi, who according to the *New York Times* has broken away from the "Committee for the Scientific Investigation of Claims of the Paranormal," has said that that group was "more preoccupied with denouncing . . . than with investigating." It is Truzzi's conviction that claims to truth in those areas normally not considered to fall within the parameters of accepted science must "not be dismissed out of hand." It is incumbent on science to be scientific about those hypotheses, "matters that present legitimate puzzle areas for science," which some scientists and also philosophers are prone to denounce before investigating. They would do well to lend their interest, their methods, their techniques, and their moral and financial support in a serious search for historical knowledge relating to the ancient astronaut hypothesis. It has too long been not only ignored but unscientifically prejudged as improbable in advance of the needed research. If that is science, then I have long misunderstood what science is.

More power to Erich von Däniken. We can only hope that his efforts will prosper.

Notes

1. Ronald Story, *The Space-Gods Revealed* (changed from *The Space-Gods Hoax* under threat of suit) (New York: Harper & Row, 1976).

2. Robert Earle, *Proof of Ancient Astronauts*, Nazca Sketchbooks I, II, III (Bay Village, Ohio: Robert Earle, I, 1975; II, 1976; III, 1978). In private correspondence, Mr. Earle has claimed that as a result of six years of study and six months of in-the-field exploration in the company of archaeologists, he has discovered the real meaning of the Nazcan runway arrows: they are an astronomical chart of asteroids and comets—on which the ancient astronauts hitched "piggyback" rides to take them to various places in their travels. No less than one-quarter of the Nazcan ground drawings, he states, are of hummingbird gods which, he insists, are depictions of hovering spacecraft.

3. Dr. Stuart W. Greenwood, aerospace engineer, member of the British Interplanetary Society, and of the American Institute of Aeronautics and Astronautics suggested the nuclear separation space in private conversation.

4. Josef F. Blumrich wrote *The Spaceships of Ezekiel* (New York: Bantam Books, 1974). His initial intent was to disprove von Däniken's thesis. Of his change of mind, he writes, "Hardly ever was a total defeat so rewarding, so fascinating, and so delightful." Blumrich is a retired engineer, former director of Systems Layout branch of NASA, who contributed to the designing and building of the Saturn V booster. He has since taken out patents for designs based on Ezekiel's "technical" descriptions.

The Ancient Astronaut Hypothesis: Science or Pseudoscience?

MARY VETTERLING-BRAGGIN

In a feisty little book titled *Chariots of the Gods?*, Erich von Däniken construes a number of ancient drawings, calendars, maps, inscriptions, texts, gigantic lines in the earth, pyramids, caves, and legends as evidence for the following hypothesis: ancient astronauts visited the earth in spaceships somewhere between 10,000 and 40,000 B.C. and mated with female[1] creatures living there. Eventually the offspring of such pairings themselves mated and evolved into the species homo sapiens. The original earth creatures obtained a knowledge of flying from these ancient astronauts which they then passed on to their descendants who, in turn, produced the calendars, maps, drawings, and so on in question.[2] The original earth creatures, according to von Däniken, viewed the ancient astronauts as "gods" and their spaceships as "chariots," thus accounting for the title of the book itself.

At several points in the book, von Däniken refers to this hypothesis as merely a possible one:

> Yet *nothing* is incredible any longer. The word "impossible" should have become literally impossible for the modern scientist. Anyone who does not accept this today will be crushed by reality tomorrow. So let us stick tenaciously to our theory, according to which astronauts from distant planets visited the earth thousands of years ago.[3]

But at other points, he argues not merely that the hypothesis is a possible one, but that it is credible enough to warrant serious scientific investigation:

> If the urge to discover our past is not sufficient incentive to set modern intensive research work in motion, perhaps the slide rule could be usefully employed. So far, at all events, no scientist has been asked to use the most modern apparatus to investigate radiation at Tiahuanaco, Sacsahuaman, the legendary Sodom, or in the Gobi Desert. Cuneiform texts and tablets from Ur, the oldest books of mankind, tell without exception of "gods" who rode in the heavens in ships, of

> "gods" who came from the stars, possessed terrible weapons and returned to the stars. Why do we not seek them out, the old "gods"? [4]

In what follows it will be asked in what senses of the term the ancient astronaut hypothesis is a "possible" one, and whether or not it is really worthy of scientific investigation. Is there any merit to von Däniken's unusual methods of justifying his hypothesis, or is this merely a case of pseudoscience at its worst?

Is the Ancient Astronaut Hypothesis "Possible"?

Let us begin with a sampling of the evidence and von Däniken's statements about it, in terms of which the possibility and scientific credibility of the ancient astronaut hypothesis are initially to be gauged. It will be of help to distinguish between the "real-world" evidence itself, von Däniken's fairly non-commital statements about the evidence, his fairly strong interpretations of the evidence, and the reasons he gives for interpreting the evidence in the way he does.

1. Evidence:	The Peri Reis maps.
Straightforward statement about the evidence:	In the eighteenth century, ancient maps were found in the Topkapi Palace in Turkey. On those maps, the continents away from the center sink downward.
Interpretation of the evidence:	These maps are copies of original photographs taken from a very great height.
Reasons for the interpretation:	Professor Charles H. Hapgood and mathematician Richard W. Strachan agree with this description. The maps are exact resemblances of photographs taken from American lunar probes.[5]
2. Evidence:	Some Peruvian cliff drawings.
Straightforward statement about the evidence:	Enormous ancient drawings have been found on cliffs in many parts of Peru.
Interpretation of the evidence:	The drawings are signals for a being floating in air.
Reasons for the interpretation:	No other purpose could have been served by the drawings. Von Däniken's own immediate reaction to one of them was that it was such a signal.[6]
3. Evidence:	Lines on the plain of Nazca.
Straightforward statement about the evidence:	In the plain of Nazca, there is a set of ancient gigantic lines. Some lines intersect, some are parallel to one another, some come to a sudden end.

Interpretation of the evidence:	The plain of Nazca is an airfield built according to instructions from an aircraft.
Reasons for the interpretation:	These lines give von Däniken the clear-cut impression of being an airfield. Measurements of the lines show that they were laid out according to astronomical plans.[7]
4. Evidence:	Sumerian astronomy.
Straightforward statement about the evidence:	Ancient Sumerians had a very sophisticated astronomy.
Interpretation of the evidence:	A knowledge of astronomy was given to the ancient Sumerians by astronauts.
Reasons for the interpretation:	There was no culture prior to our own sophisticated enough to obtain this knowledge by itself.[8]

These are taken more or less at random from von Däniken's work. The list of evidential statements and justifications is considerably expanded in his work as a whole (von Däniken tries to convince by sheer bombardment), but this sampler will suffice for our purposes.

Now in claiming that the ancient astronaut hypothesis (call it h_1) is a "possible" one in terms of his evidence, von Däniken may be making a modest claim, namely, that (a) it is not self-contradictory, and (b) none of the evidence and none of *his*[9] straightforward statements about the evidence rule it out. An examination of all the evidence and all the straightforward statements about the evidence presented in his work shows this claim to be true; none of it rules out von Däniken's hypothesis and it is clearly not logically impossible that h_1 is true. But any number of other hypotheses are also "possible" in this sense of the term (call it "possible$_1$"):

(h_1) Ancient people who had a knowledge of flying created the maps, drawings, lines, sophisticated astronomy, and so on. This knowledge was obtained from extraterrestrial beings who flew to earth in spaceships.
(h_2) Ancient people who had no knowledge of flying created the maps, drawings, lines, sophisticated astronomy, and so on.
(h_3) Ancient people who themselves developed a knowledge of flying created the maps, drawings, lines, sophisticated astronomy, and so on.
(h_4) Ancient extraterrestrial astronauts came to earth and created the maps, drawings, lines, sophisticated astronomy, and so on themselves.
(h_5) The Good Witch of the East created the maps, drawings, lines, sophisticated astronomy, and so on.
(h_6) Angels fell from heaven and created the maps, drawings, lines, sophisticated astronomy, and so on.

The list, of course, is endless.

So the claim that the ancient astronaut hypothesis is "possible$_1$," although true, turns out to be relatively uninteresting from a scientific point of view.

The scientist, who is limited by time, energy, and money, must be able to distinguish among the (potentially infinite) relevant hypotheses in order to determine which are worthy of further investigation. Mere possibility$_1$ fails to distinguish flights of fancy from serious scientific proposals, or daydreams from viable research projects.

But perhaps h_1 is the only "possible" candidate if the term is construed in a stronger sense (call it "possible$_2$"), a sense in which not just any flight of fancy would be "possible." Here, we might suggest that a hypothesis is "possible$_2$" if and only if (a) it is not self-contradictory, and (b) none of the evidence as von Däniken[10] interprets it rules it out. Is h_1 "possible$_2$"? Are the other hypotheses (particularly h_2, the one currently held by most archaeologists and the one von Däniken personally finds most objectionable[11]) not "possible$_2$"?

A quick review of von Däniken's interpretive statements regarding the evidence shows this much, at least, to be so: if his interpretive statements are themselves true, they do indeed rule out every hypothesis except h_1 (in which case h_1 is the only "possible$_2$" candidate available). But if von Däniken's interpretive statements are false, we need not worry about what they rule out, nor need we be impressed that h_1 alone is "possible$_2$" against a background of illegitimate interpretations.

The trouble here is that there is little reason to believe that these interpretive statements are true. That they are indeed suspect can be seen by considering some examples. Von Däniken takes the ancient Turkish maps to be copies of original photographs taken from a very great height. (This interpretation conveniently rules out the possibility of the maps having been drawn up by people who did not either themselves fly or know someone who did.) He seems to justify this interpretation by appeal to the testimony of a professor and a mathematician and by claiming an exact resemblance between the Turkish maps and photographs taken aboard a modern spaceship. But he unfortunately gives no reason to suppose that either the professor or the mathematician is an expert in such matters, or that they are not mistaken, or that their views are in general agreement with those of persons known to be experts in such matters. And the resemblance between the maps and actual aerial photographs of the earth can hardly be called "exact."[12]

In a similar vein, von Däniken interprets the evidence that ancient Sumerians had a knowledge of astronomy as being evidence that the Sumerians got the knowledge from ancient astronauts (which conveniently entails the view that there *were* astronauts). He seems to justify this interpretation on the ground of his belief (patently false—witness, for example, the ancient Egyptian culture, among others) that no culture prior to our own was sophisticated enough to have obtained a knowledge of astronomy on its own.

This type of analysis can be carried out for every interpretation von Däniken makes of the evidence (the exercise will be left up to the reader) and

with precisely the same result: the interpretive statements, if true, *would* rule out the hypotheses other than h_1, thereby making h_1 itself the only "possible$_2$" candidate available. But there seems little reason to prefer h_1 over the other relevant hypotheses, or for that matter to concern ourselves with "possibility$_2$"—defined in terms of these dubious interpretations—at all.

Some make the stronger claim that many of von Däniken's interpretations are downright false because they conflict with other "facts" known to be true. Clifford Wilson, for example, argues that the plain of Nazca is not an airfield because, among other reasons, the "parking area" described by von Däniken is not large enough for a spaceship landing.[13] But Wilson's "facts" are just as much in need of confirmation as are von Däniken's interpretations. How does Wilson know how large an ancient astronaut would have been, or how big a spaceship would have been, or how big a spaceship he, she, or it would have required, or how much room that ship would have needed to land? Others[14] make the more modest claim that although many of von Däniken's interpretations are not definitely known to be false, there is good reason to believe that they are. Such persons provide "facts" that speak against the interpretations and that, if not exactly known for certain to be true, certainly are much more strongly confirmed than are Wilson's.

About the best that can be said for the ancient astronaut hypothesis so far is that it is as "possible" in the weaker sense outlined as is any other relevant hypothesis. Were von Däniken's interpretive statements regarding the evidence true, it would of course be significant that h_1, but none of the other hypotheses, is "possible" in a stronger sense defined in terms of coherency with such interpretations. Although these interpretive statements are not definitively proven either true or false, there does appear to be little reason, on the basis of von Däniken's justifications, for accepting them, good reason, on the basis of "facts" provided by some critics, not to accept them, and thus no reason to hold that h_1 alone is "possible" in any significant stronger sense of that term.

Is the Ancient Astronaut Hypothesis Worthy of Scientific Investigation?

At times, von Däniken appears to be suggesting not only that h_1 is the only hypothesis not ruled out by his statements about the evidence on hand, but that h_1 is actually credible enough to warrant serious scientific investigation. In a quote cited at the beginning of this paper, for example, he says that h_1 is worth investigating because it is confirmed by the statement "the oldest books of mankind . . . tell . . . of 'gods' who rode in the heavens" (among others). Does this evidential statement in particular confirm the ancient astronaut hypothesis? And if it does, would it or any of the other evidential statements von Däniken presents constitute sufficient justification for a scientific investigation of h_1?

A hypothesis *h* can be directly confirmed by an evidential statement or set of evidential statements *e* in one of two ways. The statement(s) *e* could be said to "confirm" *h* in a weak sense in which *e* provides *some* support for *h* (raises *h*'s degree of probability by some small increment). Or, *e* could be said to "confirm" *h* in a strong sense in which it provides *good* support for *h* (raises *h*'s degree of probability high enough [15] to warrant serious consideration).[16]

To take an example, the evidential statement "this swan is white" weakly confirms the hypothesis "all swans are white" in that it provides some support for that hypothesis, but it does not provide much support for it; all the swans other than this one might be black. Likewise, the straightforward evidential statement "the oldest books of mankind tell of 'gods' who rode in the heavens" weakly confirms h_1 in that it provides some support for it. But it surely does not constitute very good support for it; the writers of these books could have been reproducing a fairy tale, speaking metaphorically of religious experiences, really talking about gods instead of astronaut-like creatures, or the like. In fact, it can be claimed that this particular straightforward statement is analogous to all the others in von Däniken's work as a whole in precisely that respect, namely, that they all weakly, but not strongly, confirm h_1. However, it could be claimed with equal plausibility that the straightforward evidential statements weakly, but not strongly, confirm every other relevant hypothesis as well. But having weakly confirmed a hypothesis surely constitutes no ground for a full-fledged scientific investigation; if it did, scientists would find themselves in the impossible position of having to investigate virtually all hypotheses (barring self-contradictory ones).

The only evidential statements von Däniken has provided that would even come close to confirming h_1 in the stronger sense of "confirm" (i.e., that would even come close to warranting a belief that h_1 is probable enough to be worth considering seriously), were they true, are his interpretive statements regarding this evidence. If they are true, then with respect to them h_1 is not only likely, it is the only available candidate. But as was argued in the first section of this paper, there is little reason to accept these interpretations in the first place, and good reason not to. There is, as a result, little reason to construe them as actually raising h_1's degree of probability (or lowering the probability of any other relevant hypothesis) at all.

However, perhaps there are some reasons not considered by von Däniken for considering some hypotheses more worthy of investigation than others, reasons that, if taken as a group, might override the negative results of our discussion of h_1's credibility with respect to von Däniken's own evidential statements. Carl Hempel has suggested five characteristics of a hypothesis that a scientist might consider in order to determine its acceptability or credibility at any given time: (1) quantity, variety, and precision of all the relevant evidence available, (2) confirmation by "new" test implications, (3) theoretical support, (4) simplicity, and (5) credibility of the hypothesis

relative to other statements generally accepted by the scientific community at the time.[17] They can briefly be explicated as follows:

Quantity, variety, and precision of the evidence: Unless there is unfavorable evidence on hand, a hypothesis is generally regarded as increasingly confirmed with the number of instances of favorable evidence. But the greater the variety and precision of the evidence, the stronger is the resulting support. That is to say, if a new bit of evidence is the result of a kind of test different from earlier tests, or is obtained through procedures of observation and measurement more precise than ones used earlier, it is usually weighted more heavily than earlier ones.

Confirmation by "new" test implications: Where new facts come to light that were not known or taken into account at the time a hypothesis was formulated and these new facts are consistent with a hypothesis, they are generally regarded as increasing the confirmation of that hypothesis.

Theoretical support: If more inclusive scientific hypotheses or theories support one hypothesis over another, in general, the hypothesis with the stronger theoretical support is regarded as the more worthy of further investigation. (Theoretical support for a hypothesis is sometimes also called "indirect confirmation" for that hypothesis.)

Simplicity: The simpler of two hypotheses constructed to account for the same evidential descriptions is generally regarded as preferable.

Probability of a hypothesis relative to a given body of knowledge: Where all the evidential statements accepted by scientists support one hypothesis over another, in general, the one supported more strongly by these statements is regarded as more acceptable than the other.

The above characteristics are not claimed to be either exhaustive or precisely formulable.[18] They serve merely as rules of thumb for the scientist who, by virtue of time, energy, and financial limitations, must be able to select from all the hypotheses on hand those that are initially acceptable and thus worthy of further research.

The ancient astronaut hypothesis does not fare very well with respect to these five characteristics, particularly the last three. It is clearly one of the most complicated of the relevant hypotheses (if true, it would require the existence of ancient astronauts *and* ancient spaceships *and* mating behavior between astronauts and earth creatures *and* the evolution of earth-astronaut creatures, etc.), it has no further theoretical support (which von Däniken himself admits[19]), and it is not credible relative to other statements generally accepted by the scientific community (for instance, that ancient people were capable of creating the evidence in question on their own and without a knowledge of flying[20]). Hypothesis h_2 does the best of those on our list with respect to the five characteristics, h_3 is in second place, and the others all do so poorly that it is hard to rank them. If we accept Hempel's suggestions for theory selection as reasonable criteria for scientific acceptability, it would

have to be concluded that it would be unwise of the scientist to begin investigating h_1 until or unless h_2 or h_3 were first investigated and found to be unsatisfactory.

However, some might wish to argue that these criteria for scientific acceptability are themselves suspect. The third and fifth criteria seem those most prone to begging the question in favor of a scientific status quo and thus most susceptible to attack from an outsider such as von Däniken. And, although h_1 still clearly comes out a loser with respect to one of the others, they are, after all, merely "intuitive" criteria; no one has yet been able to give a precise formulation for any of them. But, unfortunately, even on some of the most liberal accounts available–barring von Däniken's own–for what would count as a "scientifically acceptable" hypothesis, h_1 would still fail to make the grade. The standard Verstehen[21] account, for example, would merely require von Däniken to have "empathetically identified with" or to have "placed himself in the shoes of" the social actors referred to in the ancient astronaut hypothesis. But not even von Däniken claims to have done that.[22] More conservative Verstehen accounts[23] would merely require von Däniken to have come up with an example of an expert in these matters who has empathized with the astronauts. But von Däniken has not done that either.[24] Were there even a small chance of von Däniken's being able to meet either of these Verstehen criteria, the ancient astronaut case might begin to look interesting from a scientific point of view. Admittedly, however, the possibility of his achieving success at such tasks is small.

Conclusion

It would appear that the ancient astronaut hypothesis is merely one of a potentially infinite number of possible hypotheses, where "possible" is very weakly construed. Moreover, the hypothesis fares much worse than at least one other relevant hypothesis (the archaeological one) when judged according to standard criteria for scientific acceptability. Although these criteria are themselves not precisely formulable, and although not all philosophers of science are willing to commit themselves to them anyway, even on some of the more sympathetic accounts available, h_1 fails to rank among the "scientifically" acceptable. This does not, of course, preclude the possibility that it will rank among them in the future, but at this time, it must inescapably be concluded that h_1 and von Däniken's "justifications" for it are both prime examples of pseudoscience at its worst.

Notes

1. This part of the hypothesis has the odd implication that the astronauts, were there any, had to have been either male creatures, hermaphroditic creatures, or unusually gifted machines. Since none of the evidence or statements about the evidence actually rules out there having been female astronauts, the implication can

probably best be explained as the product of an imagination healthy in all respects but one, as opposed to any real or intended exclusion of females from the realm of the possible.

2. Erich von Däniken, *Chariots of the Gods*? trans. Michael Heron (New York: Putnam's, 1970), pp. 70–71. His later works on the same subject include *In Search of Ancient Gods* (New York: Putnam's, 1974), *Gods from Outer Space* (New York: Putnam's, 1971), *The Gold of the Gods* (New York: Putnam's, 1973), *Miracle of the Gods* (New York: Dell, 1976), and *Von Däniken's Proof* (New York: Bantam Books, 1978).

3. Ibid., p. 47.

4. Ibid., p. 40.

5. Ibid., p. 32.

6. Ibid., pp. 33–34.

7. Ibid., p. 32.

8. Ibid., pp. 41–42.

9. Von Däniken's "possibility" claim is being interpreted very sympathetically here by virtue of the insertion of the word "his." If von Däniken really means that none of *the* straightforward evidence available rules out h_1, his claim is clearly both less modest and probably false. For at least some of the straightforward descriptions available are those of archaeologists which, it might plausibly be argued, do rule out h_1. At the very least, they make h_1 highly unlikely.

10. Once again (see footnote 9), the insertion of the reference to von Däniken himself makes this a very weak construal of "possibility."

11. At several points in the book von Däniken launches into virulent attacks on archaeological theories and hypotheses. See, for example, pp. 9, 29, 32.

12. In this connection Clifford Wilson says: "Though the map is indeed a remarkable achievement of the Renaissance period, it is not nearly as accurate as is claimed by von Däniken. Nearly a thousand miles of coast is missing from east South America, and what he claims is Antarctica is a land mass joined directly to South America—the ocean between is ignored. A space photograph certainly would not produce such a result.

"It is at least debatable to suggest that an aerial photograph over Cairo could ever include the Antarctic Continent. Nor would an actual photograph make a total error of about five per cent in the size of the land masses." (*Crash Go the Chariots* [San Diego, Calif.: Master Books, 1976], p. 78.)

13. Ibid., pp. 14–17.

14. See the transcript of a NOVA television program titled "The Case of the Ancient Astronauts." The transcript is available from NOVA Transcripts, Box 1000, Boston, Mass. 02118 for a prepaid fee of $3.00.

15. This, of course, raises the question of how high "high enough" is. On most accounts, it hovers somewhere around ½. But I shall appeal to the reader's intuition in this case, hoping that he or she will agree that even if it is a good deal lower than ½, it is still not met by von Däniken's statements about the evidence. For a full discussion about the degree of confirmation required in science, see Richard Rudner, "The Scientist qua Scientist Makes Value Judgements," in *Readings in the Philosophy of Science*, ed. Baruch Brody (Englewood Cliffs, N.J.: Prentice-Hall, 1970). See also Isaac Levi, "Must the Scientist Make Value Judgements?" and Richard C. Jeffrey, "Valuation and Acceptance of Scientific Hypotheses," both in the same volume.

16. "Warranting serious consideration" is construed here as at least a necessary condition for a hypothesis's being worthy of a full-fledged scientific investigation, even if it is not sufficient.

17. For a full explication of these criteria, complete with examples of their application to specific cases, see Carl G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1966), pp. 33–46.

18. Ibid., pp. 45–46.

19. Von Däniken, *Chariots*, p. 9.

20. See the NOVA transcript.

21. This view is presented, for example, in Carl Weber's *Basic Concepts in Sociology*, trans. H.P. Secher (New York: Greenwood Press, 1962). Peter Winch, in his *The Idea of Social Science and Its Relation to Philosophy* (New York: Humanities Press, 1958), also argues for this view. His position differs from Weber's, however, in his interpretation of the term "meaningfulness." For Weber, a social-scientific event is "meaningful" when it is one with which the social scientist can empathetically identify. For Winch, it is "meaningful" when it is rule-governed. According to this latter view, in order for the social scientist to understand a hypothesis, he must understand the rules that provide the criteria specifying for the social actor(s) the relevant features of their situation. But Winch, like Weber, has failed definitively to show that this type of Verstehen is actually necessary for "scientific" activity.

22. Not only does von Däniken not claim to have "empathetically identified with" the astronauts, he does not even claim to have "empathetically identified with" the ancient earth creatures that were supposed to have come into contact with them. The closest he comes to empathetic identification at all is in his "immediate reactions" to the evidence he claims was created by the descendants of such creatures. I am grateful to Patrick Grim for this point.

23. For an explication and defense of this position, see Michael Scriven, "Logical Positivism and the Behavioral Sciences," in *The Legacy of Logical Positivism*, ed. Peter Achinstein and Stephen Barker (Baltimore: Johns Hopkins University Press, 1969), and "Verstehen Again," *Theory and Decision* 1 (1971). For a criticism of the view, see Howard Cohen, "*Das Verstehen* and Historical Knowledge," *American Philosophical Quarterly* 10 (1973): 299–306. For a response to Cohen's views, see Michael Martin, "Justification by Verstehen," in Martin, *Social Science and Philosophical Analysis: Essays in the Philosophy of the Social Sciences* (Washington, D.C.: University Press of America, 1978), pp. 355–368.

24. Von Däniken does make reference to a professor and a mathematician whom he claims agree with his views. Even on the presumption that they actually do, it is not made clear in von Däniken's work that they are experts at *this* topic, even though they might be at some other topic.

Suggested Readings

As noted in the general introduction, this is not intended as anything like a complete bibliography. The works mentioned are generally standard sources, for the most part easily available, which may be of help at the next stage of the reader's research.

Standard presentations of UFO reports:

Condon, E. U., scientific director, and Gillmor, Daniel S., ed. *Final Report of the Scientific Study of Unidentified Flying Objects Conducted by the University of Colorado under Contract to the United States Air Force.* New York: E. P. Dutton, 1969.

Story, Ronald D. *The Encyclopedia of UFO's.* Garden City, N.Y.: Doubleday, 1980. Impressively comprehensive.

Story, Ronald D., with Greenwell, J. Richard. *UFOs and the Limits of Science.* New York: Morrow, 1981.

Symposia:

Fuller, Curtis G., ed. *Proceedings of the First International UFO Congress.* New York: Warner Books, 1980.

Sagan, Carl, and Page, Thornton, eds. *UFO's: A Scientific Debate.* New York: W. W. Norton, 1972.

Critical works concerning UFOs:

Klass, Philip J. *UFOs Explained.* New York: Random House, 1974.

———. *UFO's–Identified.* New York: Random House, 1968.

Vallee, Jacques, and Vallee, Janine. *The UFO Enigma: Challenge to Science.* New York: Ballantine Books, 1966. A good attempt at a sophisticated and evenhanded treatment, mildly critical in tone.

The ancient astronaut hypothesis:

Blumrich, Josef F. *The Spaceships of Ezekiel.* New York: Bantam Books, 1974.

Sitchin, Zecharia. *The Twelfth Planet.* New York: Avon Books, 1978.

Von Däniken, Erich. *Chariots of the Gods?* Translated by Michael Heron, New York: Putnam's, 1970.

———. *Von Däniken's Proof.* New York: Bantam Books, 1978. The latest of the series.

Critiques of ancient astronauts:
Story, Ronald D. *The Space-Gods Revealed: A Close Look at the Theories of Erich von Däniken.* New York: Harper & Row, 1980.
Wilson, Clifford. *Crash Go the Chariots.* Rev. ed. San Diego, Calif.: CLP, 1976.
———. *The Chariots Still Crash.* New York: New American Library, 1976.

SECTION V

OTHER APPROACHES TO THE OCCULT

Introduction

The problem of demarcation—how science is to be distinguished from pseudoscience—has been a central issue in our examination of various topics that might loosely be included within "the occult." But it might be that in dealing with the occult in this way we have done at least some aspects of the occult a disservice, or have been unfair to some pursuits or traditions that it includes. This final section is intended to broach some more general questions of this kind regarding science and other endeavors.

A relatively weak claim that might be made in this regard is that to decide that something is not science is not necessarily to condemn it. Art, music, and aesthetic and ethical values clearly do not qualify as science, but are in no way inferior for that. There are things of value other than science, and there are perhaps pursuits of greater value. In dealing with the occult with scientific status so much in mind, we might be led to ignore other traditions valuable in other regards. Perhaps ethical and aesthetic values are embodied in some aspects of the occult to which scientific status or the lack of it is simply irrelevant.

Often, however, a stronger claim is made; that science has equivalent or superior rivals within the occult. In the first piece of this section, WILLIAM JAMES comes close to making such a claim in discussing mysticism and the effects of nitrous oxide. ". . . our normal waking consciousness," James maintains, ". . . is but one special type of consciousness, whilst all about it, parted from it by the filmiest of screens, there are potential forms of consciousness entirely different." James attempts to be scrupulously fair to the claims of mysticism, and concludes that although they can wield no special authority, they may nevertheless reveal "the truest of insights into the meaning of this life."

In the second piece, EDWARD CONZE makes a much more explicit attack on science in behalf of the occult. Conze condemns "science-bound

philosophers" for their "ruthless will for boundless power," and argues that the methods of science are "useless for the exploration of two-thirds of the universe." Here the reader should attempt to separate carefully different arguments and appeals that Conze makes. It is also important to distinguish points at which Conze is arguing for values other than those exhibited in science from points at which he is arguing for the occult as in some other way a rival to science.

In the third piece of the section, I attempt to raise some questions concerning science and values and to reply in part to some common attacks on science made not only by Conze but by Carlos Castaneda, Alan Watts, Philip Slater, Colin Wilson, and others. Here I attempt to distinguish essential from non-essential values in science, and to argue that although science is "value-laden" it does not follow that science is on all fours with conflicting value systems or with rival "ways of knowledge" within the occult. Although this is the final piece of the collection, it is of course not intended as anything like the last word.

The discussion in this final section raises philosophical issues broader than those addressed in the volume as a whole. For the most part we have concentrated on issues within the philosophy of science. But James's and Conze's pieces raise the quite general issue of epistemological relativism, and my own piece touches on important questions concerning science and values. This is, I think, entirely appropriate as a brief indication of the broader philosophical settings of the particular issues with which we have dealt.

Notes on Mysticism and Nitrous Oxide

WILLIAM JAMES

Nitrous oxide and ether, especially nitrous oxide, when sufficiently diluted with air, stimulate the mystical consciousness in an extraordinary degree. Depth beyond depth of truth seems revealed to the inhaler. This truth fades out, however, or escapes, at the moment of coming to; and if any words remain over in which it seemed to clothe itself, they prove to be the veriest nonsense. Nevertheless, the sense of a profound meaning having been there persists; and I know more than one person who is persuaded that in the nitrous oxide trance we have a genuine metaphysical revelation.

Some years ago I myself made some observations on this aspect of nitrous oxide intoxication, and reported them in print. One conclusion was forced upon my mind at the time, and my impression of its truth has ever since remained unshaken. It is that our normal waking consciousness, rational consciousness as we call it, is but one special type of consciousness, whilst all about it, parted from it by the filmiest of screens, there lie potential forms of consciousness entirely different. We may go through life without suspecting their existence; but apply the requisite stimulus, and at a touch they are there in all their completeness, definite types of mentality which probably somewhere have their field of application and adaptation. No account of the universe in its totality can be final which leaves these other forms of consciousness quite disregarded. How to regard them is the question—for they are so discontinuous with ordinary consciousness. Yet they may determine attitudes though they cannot furnish formulas, and open a region though they fail to give a map. At any rate, they forbid a premature closing of our accounts with reality. Looking back on my own experiences, they all converge towards a kind of insight to which I cannot help ascribing some metaphysical significance. The keynote of it is invariably a reconciliation. It is

Excerpted from "Mysticism," lectures XVI and XVII of William James, *The Varieties of Religious Experience* (New York: New American Library, 1958).

as if the opposites of the world, whose contradictoriness and conflict make all our difficulties and troubles, were melted into unity. Not only do they, as contrasted species, belong to one and the same genus, but *one of the species*, the nobler and better one, *is itself the genus, and so soaks up and absorbs its opposite into itself.* This is a dark saying, I know, when thus expressed in terms of common logic, but I cannot wholly escape from its authority. I feel as if it must mean something, something like what the Hegelian philosophy means, if one could only lay hold of it more clearly. Those who have ears to hear, let them hear; to me the living sense of its reality only comes in the artificial mystic state of mind. . . .

I have now sketched with extreme brevity and insufficiency, but as fairly as I am able in the time allowed, the general traits of the mystic range of consciousness. *It is on the whole pantheistic and optimistic, or at least the opposite of pessimistic. It is anti-naturalistic, and harmonizes best with twice-bornness and so-called other-wordly states of mind.*

My next task is to inquire whether we can invoke it as authoritative. Does it furnish any *warrant for the truth* of the twice-bornness and supernaturality and pantheism which it favors? I must give my answer to this question as concisely as I can.

In brief my answer is this—and I will divide it into three parts:—

(1) Mystical states, when well developed, usually are, and have the right to be, absolutely authoritative over the individuals to whom they come.

(2) No authority emanates from them which should make it a duty for those who stand outside of them to accept their revelations uncritically.

(3) They break down the authority of the non-mystical or rationalistic consciousness, based upon the understanding and the senses alone. They show it to be only one kind of consciousness. They open out the possibility of other orders of truth, in which, so far as anything in us vitally responds to them, we may freely continue to have faith.

I will take up these points one by one.

1

As a matter of psychological fact, mystical states of a well-pronounced and emphatic sort *are* usually authoritative over those who have them.[1] They have been "there," and know. It is vain for rationalism to grumble about this. If the mystical truth that comes to a man proves to be a force that he can live by, what mandate have we of the majority to order him to live in another way? We can throw him into a prison or a madhouse, but we cannot change his mind—we commonly attach it only the more stubbornly to its beliefs.[2] It mocks our utmost efforts, as a matter of fact, and in point of logic it absolutely escapes our jurisdiction. Our own more "rational" beliefs are based on evidence exactly similar in nature to that which mystics quote for theirs. Our senses, namely, have assured us of certain states of fact; but mystical ex-

periences are as direct perceptions of fact for those who have them as any sensations ever were for us. The records show that even though the five senses be in abeyance in them, they are absolutely sensational in their epistemological quality, if I may be pardoned the barbarous expression—that is, they are face to face presentations of what seems immediately to exist.

The mystic is, in short, *invulnerable*, and must be left, whether we relish it or not, in undisturbed enjoyment of his creed. Faith, says Tolstoy, is that by which men live. And faith-state and mystic state are practically convertible terms.

2

But I now proceed to add that mystics have no right to claim that we ought to accept the deliverance of their peculiar experiences, if we are ourselves outsiders and feel no private call thereto. The utmost they can ever ask of us in this life is to admit that they establish a presumption. They form a consensus and have an unequivocal outcome; and it would be odd, mystics might say, if such a unanimous type of experience should prove to be altogether wrong. At bottom, however, this would only be an appeal to numbers, like the appeal of rationalism the other way; and the appeal to numbers has no logical force. If we acknowledge it, it is for "suggestive," not for logical reasons: we follow the majority because to do so suits our life.

But even this presumption from the unanimity of mystics is far from being strong. In characterizing mystic states as pantheistic, optimistic, etc., I am afraid I over-simplified the truth. I did so for expository reasons, and to keep the closer to the classic mystical tradition. The classic religious mysticism, it now must be confessed, is only a "privileged case." It is an *extract*, kept true to type by the selection of the fittest specimens and their preservation in "schools." It is carved out from a much larger mass; and if we take the larger mass as seriously as religious mysticism has historically taken itself, we find that the supposed unanimity largely disappears. To begin with, even religious mysticism itself, the kind that accumulates traditions and makes schools, is much less unanimous than I have allowed. It has been both ascetic and antinomianly self-indulgent within the Christian church.[3] It is dualistic in Sankhya, and monistic in Vedanta philosophy. I called it pantheistic; but the great Spanish mystics are anything but pantheists. They are with few exceptions non-metaphysical minds, for whom "the category of personality" is absolute. The "union" of man with God is for them much more like an occasional miracle than like an original identity.[4] How different again, apart from the happiness common to all, is the mysticism of Walt Whitman, Edward Carpenter, Richard Jefferies, and other naturalistic pantheists, from the more distinctively Christian sort.[5]

The fact is that the mystical feeling of enlargement, union, and emancipation has no specific intellectual content whatever of its own. It is capable

of forming matrimonial alliances with material furnished by the most diverse philosophies and theologies, provided only they can find a place in their framework for its peculiar emotional mood. We have no right, therefore, to invoke its prestige as distinctively in favor of any special belief, such as that in absolute idealism, or in the absolute monistic identity, or in the absolute goodness, of the world. It is only relatively in favor of all these things—it passes out of common human consciousness in the direction in which they lie.

So much for religious mysticism proper. But more remains to be told, for religious mysticism is only one half of mysticism. The other half has no accumulated traditions except those which the text-books on insanity supply. Open any one of these, and you will find abundant cases in which "mystical ideas" are cited as characteristic symptoms of enfeebled or deluded states of mind. In delusional insanity, paranoia, as they sometimes call it, we may have a *diabolical* mysticism, a sort of religious mysticism turned upside down. The same sense of ineffable importance in the smallest events, the same texts and words coming with new meanings, the same voices and visions and leadings and missions, the same controlling by extraneous powers; only this time the emotion is pessimistic: instead of consolations we have desolations; the meanings are dreadful; and the powers are enemies to life. It is evident that from the point of view of their psychological mechanism, the classic mysticism and these lower mysticisms spring from the same mental level, from that great subliminal or transmarginal region of which science is beginning to admit the existence, but of which so little is really known. That region contains every kind of matter: "seraph and snake" abide there side by side. To come from thence is no infallible credential. What comes must be sifted and tested, and run the gauntlet of confrontation with the total context of experience, just like what comes from the outer world of sense. Its value must be ascertained by empirical methods, so long as we are not mystics ourselves.

Once more, then, I repeat that non-mystics are under no obligation to acknowledge in mystical states a superior authority conferred on them by their intrinsic nature.[6]

3

Yet, I repeat once more, the existence of mystical states absolutely overthrows the pretension of non-mystical states to be the sole and ultimate dictators of what we may believe. As a rule, mystical states merely add a supersensuous meaning to the ordinary outward data of consciousness. They are excitements like the emotions of love or ambition, gifts to our spirit by means of which facts already objectively before us fall into a new expressiveness and make a new connection with our active life. They do not contradict these facts as such, or deny anything that our senses have

immediately seized.[7] It is the rationalistic critic rather who plays the part of denier in the controversy, and his denials have no strength, for there never can be a state of facts to which new meaning may not truthfully be added, provided the mind ascend to a more enveloping point of view. It must always remain an open question whether mystical states may not possibly be such superior points of view, windows through which the mind looks out upon a more extensive and inclusive world. The difference of the views seen from the different mystical windows need not prevent us from entertaining this supposition. The wider world would in that case prove to have a mixed constitution like that of this world, that is all. It would have its celestial and its infernal regions, its tempting and its saving moments, its valid experiences and its counterfeit ones, just as our world has them; but it would be a wider world all the same. We should have to use its experiences by selecting and subordinating and substituting just as is our custom in this ordinary naturalistic world; we should be liable to error just as we are now; yet the counting in of that wider world of meanings, and the serious dealing with it, might, in spite of all the perplexity, be indispensable stages in our approach to the final fullness of the truth.

In this shape, I think, we have to leave the subject. Mystical states indeed wield no authority due simply to their being mystical states. But the higher ones among them point in directions to which the religious sentiments even of non-mystical men incline. They tell of the supremacy of the ideal, of vastness, of union, of safety, and of rest. They offer us *hypotheses*, hypotheses which we may voluntarily ignore, but which as thinkers we cannot possibly upset. The supernaturalism and optimism to which they would persuade us may, interpreted in one way or another, be after all the truest of insights into the meaning of this life.

Notes

1. I abstract from weaker states, and from those cases of which the books are full, where the director (but usually not the subject) remains in doubt whether the experience may not have proceeded from the demon.

2. Example: Mr. John Nelson writes of his imprisonment for preaching Methodism: "My soul was as a watered garden, and I could sing praises to God all day long; for he turned my captivity into joy, and gave me to rest as well on the boards, as if I had been on a bed of down. Now could I say, 'God's service is perfect freedom,' and I was carried out much in prayer that my enemies might drink of the same river of peace which my God gave so largely to me." Journal, London, no date, p. 172.

3. Ruysbroeck, in the work which Maeterlinck has translated, has a chapter against the antinomianism of disciples. H. Delacroix's book (Essai sur le mysticisme spéculatif en Allemagne au XIVme Siècle, Paris, 1900) is full of antinomian material. Compare also A. Jundt: Les Amis de Dieu au XIV Siècle, Thèse de Strasbourg, 1879.

4. Compare Paul Rousselot: Les Mystiques Espagnols, Paris, 1869, ch. xii.

5. See Carpenter's Towards Democracy, especially the latter parts, and Jefferies's wonderful and splendid mystic rhapsody, The Story of My Heart.

6. In chapter i. of book ii. of his work Degeneration, "Max Nordau" seeks to undermine all mysticism by exposing the weakness of the lower kinds. Mysticism for him means any sudden perception of hidden significance in things. He explains such perception by the abundant uncompleted associations which experiences may arouse in a degenerate brain. These give to him who has the experience a vague and vast sense of its leading further, yet they awaken no definite or useful consequent in his thought. The explanation is a plausible one for certain sorts of feeling of significance; and other alienists (Wernicke, for example, in his Grundriss der Psychiatrie, Theil ii., Leipzig, 1896) have explained "paranoiac" conditions by a laming of the association-organ. But the higher mystical flights, with their positiveness and abruptness, are surely products of no such merely negative condition. It seems far more reasonable to ascribe them to inroads from the subconscious life, of the cerebral activity correlative to which we as yet know nothing.

7. They sometimes add subjective *audita et visa* to the facts, but as these are usually interpreted as transmundane, they oblige no alteration in the facts of sense.

Tacit Assumptions

EDWARD CONZE

The mutual incomprehension of Eastern and Western philosophy has often been deplored. If there is even no contact between "empiricist" European philosophy on the one side, and that of the Vedānta and Mahāyāna on the other, it may be because they presuppose two different systems of practice as their unquestioned foundations–science the one, and yogic meditation the other. From the outset all philosophers must take for granted some set of practices, with specific rules and aims of their own, which they regard both as efficacious and as avenues to worthwhile reality.

It is, of course, essential to grasp clearly the difference between sets of practices, or "bags of tricks" which regularly produce certain results, and the theoretical superstructures which try to justify, explain and systematize them. The techniques concern what happens when this or that is done. The theories deal with the reasons why that should be so, and the meaning of what happens. However gullible and credulous human beings may be about speculative tenets, about practical issues they are fairly hard-headed, and unlikely to persuade themselves over any length of time that some technique "works" when it does not.

Yogic meditation, to begin with, demands that certain things should be *done*. There are the well-known breathing exercises, which must be performed in certain definite bodily postures. Certain foods and drugs must be avoided. One must renounce nearly all private possessions, and shun the company of others. After a prolonged period of physical drill has made the body ready for the tasks ahead, and after some degree of contentment with the conditions of a solitary, beggarly and homeless life has been achieved, the mind is at last capable of doing its proper yogic work. This consists in

systematically withdrawing attention from the objects of the senses.[1] And what could be the aim and outcome of this act of sustained introversion—so strikingly dramatized by Bodhidharma sitting for nine years cross-legged and immobile in front of a grey wall? All the adepts of Yoga, whatever their theological or philosophical differences, agree that these practices result in a state of inward tranquility (*śamatha*).

Many of our contemporaries, imprisoned in what they describe as "common sense," quite gratuitously assume, as "self-evident," that all the contents of mental life are derived from contact with external sense-data. They are therefore convinced that the radical withdrawal from those sense-data can but lead to some kind of vague vacuity almost indistinguishable from sleep or coma. More than common sense is needed to discover that it leads to a state which the Indian yogins, who under the influence of Sanskrit grammar were almost obsessed with a desire for terminological precision, called one of "tranquillity," full of ease, bliss and happiness. Likewise a Bornean Dayak must find it difficult to believe that hard, black coal can be changed into bright light within an electric bulb. There is ultimately only one way open to those who do not believe the accounts of the yogins. They will have to repeat the experiment—in the forest, not the laboratory—they will have to do what the yogins say should be done, and see what happens. Until this is done, disbelief is quite idle, and on a level with a pygmy's disbelief in Battersea power station, maintained by a stubborn refusal to leave the Congo basin, and to see for himself whether it exists and what it does. In other words, it seems to me quite unworthy of educated people to deny that there exists a series of technical practices, known as Yoga, which, if applied intelligently according to the rules, produces a state of tranquillity.[2]

So much about the technical substructure. The ideological superstructure, in its turn, consists of a number of theoretical systems, by no means always consonant with each other. Theologically they are Hindu, Buddhist or Jain. Some are atheistic, some polytheistic, others again henotheistic. Philosophically some, like Vaiśesika and Abhidharma, are pluralistic, others, like Vedānta and Mādhyamikas, monistic. These two monistic systems, again, seem to be diametrically opposed in their most fundamental tenets—the one claiming that the Self (*ātman*) is the only reality, the other that it is just the absence of a self (*nairātmya*) which distinguishes true reality from false appearance.

On closer study these disagreements do, however, turn out to be fairly superficial. All these "yogic" philosophies differ less among themselves than they differ from the non-yogic ones. They not only agree that yogic practices are valid, but in addition postulate that these practices are the avenues to the most worthwhile knowledge of true reality, as well as a basis for the most praiseworthy conduct, and that, as the source of ultimate certainty, the yogic vision itself requires no justification. Only in a state of yogic receptivity are

we fit and able to become the recipients of ultimate truth. Observations made in any other condition concern an illusory world, largely false and fabricated, which cannot provide a standard for judging the deliverances of the yogic consciousness.

A closely analogous situation prevails in Western Europe with regard to science. In this field also we can distinguish between the technology itself and its theoretical developments. The prestige of the scientific approach among our modern philosophers seems to me entirely due to its applications. If a philosopher assures us that all the "real" knowledge we possess is due to science, that science alone gives us "news about the universe"—what can have led him to such a belief? He must surely have been dazzled by the practical results, by the enormous increase in power which has sprung from the particular kind of knowledge scientists have evolved. Without these practical consequences, what would all these scientific theories be? An airy bubble, a diversion of otherwise unoccupied mathematicians, a fanciful mirage on a level with *Alice in Wonderland.* As a result of science, considerable changes have recently occurred in the material universe. Although by no means "more enduring than brass," the monuments to science are nevertheless rather imposing—acres of masonry, countless machines of startling efficiency, travel speeded up, masses of animals wiped out, illnesses shortened, deaths postponed or accelerated, and so on. This scientific method demonstrably "works," though not in the sense that it increases our "tranquillity"—far from it. All that it does is to increase "man's" power to control his "material environment," and that is something which the yogic method never even attempted. Scientific technology indeed promises limitless power, unlimited in the sense that by itself it places no limitations, moral or otherwise, on the range of its conquests. Very little notice would presumably be taken of the thought-constructions of our scientists if it were not for their impressive practical results. Dean Swift's *Voyage to Laputa* would then voice the general attitude, including that of the majority of philosophers.

As with Yoga, the bare technology is also here clothed in numerous theories, hypotheses, concepts and philosophical systems, capable of considerable disagreement among themselves. But all scientific philosophies agree that scientific research, based on the experimental observation of external objects,[3] is the key to all worthwhile knowledge and to a rational mode of life.

But though I were to speak with the tongues of angels, my "empiricist" friends will continue to shrug their shoulders at the suggestion that Yoga and other non-scientific techniques should be taken seriously. As professed "humanists" they might be expected to have a greater faith in the depth and breadth of the human spirit and its modalities. As "empiricists" they might have a more catholic notion of "experience," and as "positivists" a clearer conception of what is, and what is not, a "verifiable" fact. And even as

"scientists" they ought to have some doubts as to whether the world of sense-bound consciousness is really the whole of reality. But alas, a staggering hypertrophy of the critical faculties has choked all the other virtues. Contemporary empiricist and positivist philosophers, in their exclusive reliance on scientific knowledge, are guilty of what Whitehead has charitably called a "narrow provincialism." Usually unfamiliar with the traditional non-scientific techniques of mankind, they are also, what is worse, quite incurious about them. At best these techniques, if noticed at all, are hastily interpreted as approximations to scientific ones, worked out by ignorant and bungling natives groping in the dark. On the wilder shores of rationalism it is even rumoured that "the poet was the primitive physicist."[4] With a shudder we pass on.

To judge all human techniques by the amount of bare "control" or "power" they produce is patently unfair. Other goals may be equally worth striving for, and men wiser than we may deliberately have turned away from the pursuit of measureless power, not as unattainable, but as inherently undesirable. A graceful submission to the inevitable is not without its attractions, either. A great deal might be said, perhaps, for not wanting more power than can be used wisely, and it is much to be feared that the "captors of an unwilling universe"[5] may end as many lion tamers have ended before them.

Of all the infinite facets of the universe, science-bound philosophers will come to know only those which are disclosed to scientific methods, with their ruthless will for boundless power and their disregard for everything except the presumed convenience of the human race, and they cannot prove, or even plausibly suggest, that this small fraction of the truth about reality is the one most worth knowing about. As for the vast potentialities of the human mind, they will bring out only those which have a survival value in modern technical civilization. Not only is it a mere fraction of the human mind that is being used, but we may well wonder whether it is the most valuable section—once we consider the ugliness, noisiness and restlessness of our cities, or the effects which the handling of machines has on workers, that of scientific tools on scientists. At present it looks as if this mode of life were sweeping everything before it. It also demonstrably sweeps away much that is valuable.

2a. Turning now to the "triple world," we find that the unanimous tradition of the Perennial Philosophy distinguishes three layers of qualitatively different facts—natural, magical, and spiritual. The constitution of man is accordingly composed of three parts, reality presents itself on three levels, and threefold is the attitude we can adopt towards events.

In man we have body-mind as the first constituent, the "soul" as the second and the "spirit" as the third. In the objective world, the first level is the body of facts which are disclosed by the senses and scientific observation, and arranged by common sense and scientific theory. The second comprises a

great variety of facts which with some justice are called "occult," because they tend to hide from our gaze. They weighed heavily with our forefathers, but are now widely derided. An example is astrology, or the study of the correspondences which may exist between the position of the celestial bodies on the one hand and the character, destiny, affinities and potentialities of people on the other. In addition this second level includes the activities of the psychic senses, such as clairvoyance, clairaudience, pre-cognition, thought transference, etc., the huge field of myths and mythical figures, the lore about ghosts and the spirits of the departed, and the working of "magic," which is said to cause effects in the physical world by means of spells and the evocation of "spirits." Thirdly, the spiritual world is an intangible, non-sensuous and disembodied reality, both one and multiple, both transcending the natural universe and immanent in it, at the same time nothing and everything, quite non-sensory as a datum and rather nonsensical as a concept. Indescribable by any of the attributes taken from sensory experience, and gained only by the extinguishing of separate individuality, it is known as "Spirit" to Christians, as "emptiness" to Buddhists, as the "Absolute" to philosophers. Here our senses are blinded, our reason baffled, and our self-interest defeated.

The three worlds can be discerned easily in our attitudes, say, to cold weather. The common-sense reaction is to light a fire, to wear warm clothing, or to take a walk. The magician relies on methods like the *gtum-mo* of the Tibetans, which are claimed to generate internal heat by means of occult procedures. They are based on a physiology which differs totally from that taught in scientific textbooks, and depend on the manipulation of three mystic "arteries" (*nadis*), which are described as channels of psychic energy, but which ordinary observation fails to detect, since they are "devoid of any physical reality."[6] Finally, the spiritual man either ignores the cold, as an unimportant, transitory and illusory phenomenon, or welcomes it, as a means of penance or of training in self-control.

Technical progress and scientific habits of thought increasingly restrict us to the natural level. Magical events and spiritual experiences have ceased to be familiar, and many people do not admit them as facts in their own right. By their own inner constitution the three realms differ in their accessibility to experience, the rules of evidence are by no means the same in all three, and each has a logic of its own. In the infinitude of the spiritual realm no particular fact can be seized upon by natural means, and everything in the magical world is marked by a certain indefiniteness, a nebulousness which springs partly from the way in which the intermediary world presents itself and partly from the uncertainties of its relation to the familiar data of the bright daylight world of natural fact. Every student of the occult knows that in this field the facts are inherently and irremediably obscure. It is impossible to come across even one magical fact which could be established in the way in

which natural facts can be verified. There is a twilight about the magical world. It is neither quite light nor quite dark, it cannot be seen distinctly, and, like a shy beast when you point a torch at it, the phenomenon vanishes when the full light is turned on.[7]

The situation becomes more desperate still when we consider the spiritual. Here it is quite impossible to ever establish any fact beyond the possibility of doubt. The Buddhists express this by saying that Nirvana is "sign-less," i.e. it is of such a nature that it cannot be recognized as such. This is really a most disconcerting thought. Spirit is non-sensuous and we have no sense-data to work on. In addition, spiritual actions are disintegrated when reflected upon. If they are not to lose their bloom, they must be performed unconsciously and automatically. Further, to be spiritual, an action must be "unselfish." It is in the nature of things quite impossible ever to prove with mathematical certainty that an action has been unselfish, because selfishness is so skillful in hiding itself, because insight into human motives is marred by self-deception, and, in any case, at any given time the motives are so numerous that no one can be sure of having got hold of all of them. I. Kant has spoken the last word on this subject when he points out that "in fact it is absolutely impossible to make out by experience with complete certainty a single case in which the maxim of an action, however right in itself, rested simply on moral grounds and on the conception of duty. Sometimes it happens that with the sharpest self-examination we find nothing beside the moral principle of duty which could have been powerful enough to move us to this or that action and to a great sacrifice; yet we cannot infer from that with certainty that it was not really some secret impulse of self-love, under the false appearance of that idea, that was the actual determining cause of the will. We like then to flatter ourselves by falsely taking credit for a noble motive, whereas in fact we can never, even by the strictest examination, get completely behind the secret springs of action; since, when the question is of moral worth, it is not with the actions which we see that we are concerned, but with those inward principles of them which we do not see."[8]

Here is one of the inescapable difficulties of the human situation. All the meaning that life may have derives from contact with the magical and spiritual world, and without such contact it ceases to be worth while, fruitful and invested with beauty. It seems rather stupid to discard the life-giving qualities of these realms simply because they do not conform to a standard of truth suited only to the natural world,[9] where to the scientist phenomena appear worthy of notice only if they are capable of repetition, public observation, and measurement. They are naturally more inaccessible to natural experience than natural things are. The methods of science, mighty and effective though they be, are useless for the exploration of two-thirds of the universe, and the psychic and spiritual worlds are quite beyond them. Other faculties within us may well reveal that which the senses fail to see. In

Buddhism faith, mystical intuition, trance and the power of transcendental wisdom are held to disclose the structure of the spiritual and intermediary worlds. No one can be said to give Buddhist thinking a fair chance if he persists in condemning these sources of knowledge out of hand as completely futile and nugatory.

2b. Next, the perennial philosophy assumes that there are definite "degrees of reality." In this book we will be told that "dharmas" are "more real" than things, the images seen in trance "more real" than the objects of sense-perception, and the Unconditioned "more real" than the conditioned. People at present can understand the difference between facts which exist and "non-facts" which do not exist. But they believe that facts, if real, are all equally real, and that qualitative distinctions between them give no sense. This is the "democratic" viewpoint in vogue at the present time, which treats all facts as equal, just as all men are said to be equal.[10] In science nothing has any "meaning," and "facts" are all you ever have.

At the time when Buddhism flourished, this would have seemed the height of absurdity. Also the leading European systems of that time, like those of Aristotle and Plotinus, took the hierarchy of levels of reality quite for granted, and were indeed entirely based upon it. The lowest degree of reality is "pure matter," the highest "pure form," and everything else lies somewhere in between. The higher degrees of reality are more solid and reliable, more intellectually satisfying, and, chief of all, they are objectively "better" than the lower, and much more worth while. *Ens et bonum convertuntur*. In consequence contact with the higher degrees of reality entails a life which is qualitatively superior to one based on contact with the lower degrees. This is what sticks in the throat of the present generation. For here we affirm that "judgments of value" are not just subjective opinions, which vary with the moods of people, or their tastes or social conditions, but that they are rooted in the structure and order of objective reality itself.[11]

If the value of life depends on contact with a high level of reality, it becomes, of course, important to ascertain what reality is in its own-being (*svabhāva*), and to be able to distinguish that from the lesser realities of comparative fiction which constitute our normal world of half-socialized experience which we have made ourselves so as to suit our own ends. To establish contact with worthwhile reality has always been the concern of the exponents of the "perennial" philosophy, i.e. of most reputable philosophers of both Europe and Asia up to about AD 1450.

About this time there began in Europe that estrangement from reality which is the starting-point of most modern European philosophy. Epistemology took the place of ontology. Where ontology was concerned with the difference between reality and appearance, epistemology concentrated on that between valid and invalid knowledge. The Occamists who set the tone for all later phases of modern philosophy asserted that things by themselves

have no relations to one another, and that a mind external and unrelated to them establishes all relations between them. Ontology as a rational discipline then lost its object and all questions concerning being *qua* being seemed to be merely verbal. Science should not concern itself with the things themselves, but with their signs and symbols, and its task is to give an account of appearances (*salvare apparentias*), without bothering about the existence *in esse et secundum rem* of its hypothetical constructions.[12] In consequence, thinkers seek for "successful fictions" and "reality" has become a mere word.

It is remarkable that 1,400 years before the Mahāyāna Buddhists had taught almost exactly the same. When they realized their estrangement from reality, they looked for a reality more real than they found around them, i.e. for the "Dharma-element" itself. Modern philosophy concludes that it is better for us to turn our backs on nebulous ideas about reality as such, and to concentrate on gaining power over the environment as it appears. Power by whom, and for whom? Here a philosophy which teaches that the particular alone exists and that universals are mere words, finds refuge in an abstraction called "man," who is somehow regarded as the highest form of rational being, and for whose benefit all these developments are said to take place. To Nāgārjuna and his followers this by itself would seem to indicate a serious logical flaw at the very basis of such doctrines.

2c. Finally, and that is much easier to understand, the hierarchical structure of reality is duplicated by and reflected in a hierarchy among the persons who seek contact with it. Like is known by like, and only the spirit can know spiritual things. In an effort to commend Buddhism to the present age, some propagandists have overstressed its rationality and its kinship with modern science. They often quote a saying of the Buddha who told the Kalamas that they should not accept anything on his authority alone, but examine and test it for themselves, and accept it only when they had themselves cognized, seen and felt it.[13] In this way the Lord Buddha finds himself conscripted as a supporter of the British philosopical tradition of "empiricism." But who can do the testing? Some aspects of the doctrine are obviously verifiable only by people who have certain rather rare qualifications. To actually verify the teaching on rebirth by direct observation, one would have to actually remember one's own previous births, an ability which presupposes the achievement of the fourth *dhyāna*, a state of trance extremely scarce and rarefied. And what width and maturity of insight would be needed to actually "know" that the decisive factor in every event is a "moral" one, or that Nirvana means the end of *all* ill! The qualifications are moreover existential, and not merely intellectual. Buddhism has much to say about the spiritual hierarchy of persons, for what someone can know and see depends on what he is. So the saint knows more than the ordinary person, and among the saints each higher grade more than the lower. In consequence, the opinions and experiences of ordinary worldlings are of little account, on a level with the

mutterings of housepainters laying down the law about Leonardo da Vinci's "Virgin of the Rocks."

Notes

1. This saying from Bhagavadgīta II 58 may well be regarded as the clue to all Yoga:

yadā smharate cāyam kūrmo'ṅgānīva sarvaśah
indriyānīndriyārthebhyas tasya prajñā pratiṣṭihitā.

"He who draws away the senses from the objects of sense on every side, as a tortoise draws in his limbs (into the shell), his intelligence is firmly set (in wisdom)" (Radhakrishnan).

2. The most comprehensive and authoritative textbook is M. Eliade, *Yoga. Immortality and Freedom*, 1958.

3. The data of introspection have given rise to much uneasiness in this scheme of things. The most logical solution seems to be that of Behaviourism, which transforms psychic events into externally observable objects.

4. K. Nott, *The Emperor's Clothes*, 1953, p. 248.

5. Quis neget esse nefas invitum prendere mundum
Et velut in semet captum deducere in orbem? (Manilius II 127–8).

6. So A. David-Neel, *With Mystics and Magicians in Tibet*, p. 203. For further information about the nadīs see G. Tucci, *Tibetan Painted Scrolls*, 1949, and S.B. Dasgupta, *An Introduction to Tāntric Buddhism*, 1950.

7. For a further discussion of the difficulties of getting hold of the facts about the "intermediary world" see my article on "The Triple World" in *The Aryan Path*, xxv 5, 1954, pp. 201–2. Very instructive also is *Times Literary Supplement* 28.1.1956 on "New Concepts of Healing."

8. I. Kant, *Metaphysics of Morals*, trsl. T.H. Abbott, 1879, p. 33.

9. There is also something mean and timid about the caution of someone who wishes everything to be established beyond any reasonable doubt, and to have it inspected again and again with myopic and distrustful eyes.

10. The structure of the universe always reflects the structure of society. Likewise it is interesting to note that those who replace ontology by epistemology are Protestants who repudiate collective or corporate authority, whereas Roman Catholics, Marxists and Buddhists believe that meaningful statements can be made about the "real being" of things.

11. In addition, of course, the very assumption of qualitative differences in the worthwhileness of life has no scientific foundation, because "science," as we know it, has no eye for quality, but only for quantity. Likewise no moral qualifications are required of scientists, and the quality of their lives is unimportant when their findings are judged.

12. For further particulars see E. Conze, *Der Satz vom Widerspruch*, 1932, pp. 205–66, and the summary in English in the *Marxist Quarterly*, 1937, pp. 115–24.

13. Majjhima Nikāya I, p. 265; Taishō Issaikyō 26, k. 54, p. 769 b. Ch. A. Moore, Buddhism and Science: Both Sides, in "Buddhism and Culture," Kyoto, 1960, p. 94.

Scientific and Other Values

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In that vague realm known as the popular imagination, science is quite frequently conceived of as something that stands apart from values. Science is thought of as value-free, and this supposed freedom from values is thought to be one of its prime virtues. On one side of a fact–value gap lies the quarrelsome morass of bickering values; arbitrary, indefinite, and perhaps merely subjective. On the other side of the gap, pristine and pure, untainted by mere values, stands the imposing edifice of science.

In what follows I hope to challenge this popular image of science and values. The claim that science is or even could be value-free, I shall argue, is simply false.

But at this point it is tempting to replace the first image of science and values with a second image, an image that itself boasts no small measure of current popularity. If science does not stand apart from the morass of values, it seems, it must be but one among many rival "value systems" bogged down in that morass. There is then nothing special to be said for science that could not be said with equal justice for any of its rivals. Any favoring of science over rival "systems" becomes a matter of personal and idiosyncratic taste, ultimately as arbitrary and subjective as any other choice of values.

This second image of science and values, I shall argue, is as wrong as the first. Science is shaped by and is essentially committed to certain values. But I hope to show that because of the *ways* in which science is shaped by values and because of the *particular* values to which it is committed, it does not follow that science is on all fours with any other "system" of values. Nor does it follow that the choice becomes arbitrary between science and Edward Conze's Buddhism, or William James's mysticism, or Carlos Castaneda's Yaqui sorcery.[1]

Is Science Value-Free?

In order to answer this question we need some sketch or outline or indication, however rough, of what we are to take as "science." But any attempt to define "science" with accuracy, or to offer necessary and sufficient conditions for properly scientific procedure, is bound to embroil us immediately in the devastating difficulties of demarcation. So let us settle, at least initially, for something less: a rough and ready characterization of science in terms of lists of claims.

We are all familiar with a large number of scientific claims; claims such as the following:

(1) Water freezes at 32° F.

(2) Most calico cats are female.

(3) Hydrogen is composed of one proton and one electron.

(4) Syphilis is caused by a spirochete.

(1) through (4) are claims currently accepted, on grounds of scientific study and research, within the scientific community. And at least one way to characterize the science of our times would be in terms of a list, indefinitely large or infinite, of all such scientific claims.

Let us thus envisage such a list of scientific claims. We need not hold that any science at any time will share these claims; it is enough that our current science can be so represented, whatever inevitable discoveries or refutations the future will bring.

With this rough and ready characterization of current science we have at least one way of trying to determine whether science is value-free. Are there any value judgments in the list we have envisaged? If so, it would appear, we have good grounds for concluding that at least *our* science is not value-free. If there are no value judgments in the list, we have at least some basis for claiming that science *is* value-free.

Consider in this regard (1) through (4) above. None of these, I think, is a very promising candidate for a "value judgment." And if our complete list of current scientific claims were composed entirely of claims like (1) through (4), we would have at least some grounds for maintaining that science *is* value-free.

But (1) through (4) are only some of the claims on our list. (5) and (6), for example, would appear to have an equal right to be included among current scientific claims:

(5) Smoking is hazardous to your health.

(6) Contemporary handling of nuclear wastes is unsafe.[2]

We can also expect to find on our list claims concerning other claims, such as (7) and (8):

(7) We have compelling evidence that smoking causes cancer.

(8) It has not yet been satisfactorily established that the use of high-sulfur coal poses a significant environmental risk.

Any list of currently accepted scientific claims that did not include claims such as (5) through (8), I think, would clearly be an inadequate and incomplete list. But it can at least be argued that claims such as (5) through (8) embody value judgments, and thus that at least our current science is not value-free.

Consider claims (5), (6), and (8). (5) invokes a notion of health and of hazards to health. But health, some have argued, is itself a term to which values are crucial. A healthy organism is one that is as it *should* be, and a hazard to health is a *threat*—something that at least prima facie ought to be avoided for the sake of *proper* functioning.[3] Consider also the notions of safety and of risk that appear in (6) and (8). To claim that a situation is unsafe is surely to claim that it jeopardizes something of *value*, and to speak of risk is to speak of probabilities of *loss*. The notions of safety and risk, then, like that of health, appear to assume a particular background of values. Were human lives held to be of no more value than lives of wood lice, we would not speak scientifically of risk and safety in the ways we do. Many of our scientific claims *do* concern risk and safety, and thus assume a background of values.

Consider also a slightly different argument. Claims (7) and (8) invoke notions of compelling evidence and of the satisfactory establishment of claims. And surely any science worth its salt will include claims of this sort concerning the establishment of other claims. But "compelling evidence" and "satisfactory establishment," it can be argued, assume the same sort of background of values as indicated above for "safety" and "risk." Richard Rudner has put this point quite elegantly:

> since no scientific hypothesis is ever completely verified, in accepting the hypothesis the scientist must make the decision that the evidence is *sufficiently* strong or that the probability is *sufficiently* high to warrant the acceptance of the hypothesis. Obviously our decision regarding the evidence and respecting how strong is "strong enough," is going to be a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis. Thus, to take a crude but easily manageable example, if the hypothesis under consideration were to the effect that a toxic ingredient of a drug was not present in lethal quantity, we would require a relatively high degree of confirmation or confidence before accepting the hypothesis—for the consequences of making a mistake here are exceedingly grave by our moral standards. On the other hand, if, say, our hypothesis stated that, on the basis of a sample, a certain lot of machine stamped belt buckles was not defective, the degree of confidence we should require would be relatively not so high. *How sure we need to be before we accept a hypothesis will depend on how serious a mistake would be.*[4]

It thus appears, on several grounds, that science is not value-free.[5] Let us state our conclusions here in as strong a form as possible. Some scientific

claims concern health, safety, hazard, and risk, and each of these seems to make sense only against a general background of values. Some scientific claims concern compelling evidence, adequate establishment, and sufficiently high probability for the acceptance of other claims. These claims, on Rudner's argument, rely on a similar background of values. But here we can also say more. The envisaged list of claims we have appealed to throughout is a list of claims currently accepted within the scientific community. If so, each claim is presumably on the list because it has been accepted on grounds of "compelling evidence," "adequate establishment," or "sufficiently high probability." Thus the inclusion of *each* item on the list, even of items as apparently dispassionate as (1) through (4), will reflect the general background of values that Rudner indicates. *Some* claims on the list will invoke values in virtue of their content; for example, those that address issues of health, risk, or sufficient establishment. But regardless of its content, the presence of *each* claim on the list will reflect its acceptability in terms of background values.

In Rudner's sense, then, *all* science is shaped by values. We might also note that in principle almost any background value might do the shaping. The scientific acceptability of a claim will depend on the prospects of accepting it or not, and on the relative values we place on those prospects. Almost any value concerns the relative merit of alternative prospects, and almost any value will rank alternatives in some way. Thus in principle almost any value might be of importance for the scientific acceptability of some claim, and in that sense almost any value could be of significance in shaping science.

We have presented in some detail an argument to the effect that science is not value-free. If this argument is adequate, it appears that the first popular image of science and values presented in our introduction is drastically misconceived.

What replies to the argument above might be made in behalf of a notion of "value-free" science? Let us briefly consider two possible replies.

First, it must be noted that the list of claims with which we have been working is merely a list of *current* scientific claims. But no argument on this basis alone, it might be argued, will suffice to show that science *could* not be value-free.

This reply does seem telling, as far as it goes, against the first part of the argument presented above. A science that did not address questions of health, hazard, safety, or risk would to that extent avoid certain questions of value. Thus science *could* escape values to some extent by confining its claims to those such as (1) through (4) and avoiding claims such as (5) through (8).[6]

But this reply does not seem adequate against the full force of Rudner's argument. Surely *any* science must accept and reject claims on the basis of adequate warrant, compelling evidence, sufficiently high probability, and the like. If any such acceptance and rejection of claims demands a background of

values, then any science must assume some such background of values. In that repsect, although we have been working with a rough characterization of *current* science, our conclusions would appear to apply as well to any science at any time.

Consider also a second possible reply. What we have taken as "science," it might be argued, is not science *proper* or science *per se* but instead the everyday management, administration, and application of science. The management of science, it might be conceded, is shaped by background values. But science per se is nevertheless value-free. The scientist qua scientist neither accepts nor rejects hypotheses; he merely assigns probabilities to various hypotheses at issue.[7]

By carefully gerrymandering a distinction between "science per se" and the mere "management" of science one could, I think, carve out as "science per se" something that was value-free. But I seriously question the point of any stipulative linguistic exercise of this type. That which one would end up idolizing as "pure science" would be a barely recognizable amputation from current practice, consisting of little more than the mindless manipulation of test tubes and the mechanical marking of charts. It would not be what we think of as science and would not be science as we know it. It would fit best the work of those technicians who know least what they are doing, if it would fit the practice of any working scientist at all, and would relegate to a category of "mere management" the most important work of every Nobel Prize Winner we have. If that is the cost of maintaining that science is value-free, we had better simply concede that it is not.

Two Types of Scientific Values

Rudner's argument shows that standard scientific notions of "adequate establishment," "compelling evidence," and "sufficiently high probability" assume a background of values. In that sense we might conclude that science is "value-laden." But here it is important to consider precisely *what* values are at issue and precisely *how* science is "laden."

Rudner's argument shows that science is shaped by a background of values. But the argument does not show that any particular set of values must do the shaping. Our current science, perhaps, relies on a particular background of values. And all science, perhaps, must rely on *some* such background. But science *could* nevertheless assume a quite different background of values.

Because we value human life to the extent that we do, and because we do not value chimpanzee life more than we do, we are more wary of accepting claims regarding lethal quantities of food additives as "adequately established" where human lives, and not merely chimpanzees, are at stake. If we valued human life less, or chimpanzee life more, our list of "adequately established" claims could be expected to shift. Similarly, if we ranked Rud-

ner's machine-stamped belt buckles as on a par with original Rembrandts, we could expect our list of acceptable claims to change. Different values would then bear on what establishment we took to be adequate establishment, and as a result different claims would be accepted as adequately established.

But would a science shaped by different values in this way be any less a science? I think not. Although *some* set of values must in this way serve as background for notions of sufficient evidence and adequate establishment, it need not be any particular set of values.[8] Were our values regarding human lives and chimpanzee lives to change tomorrow, which claims we found scientifically acceptable or sufficiently warranted might change as well. But that would not render our current work with either humans or chimpanzees unscientific.

One moral to draw from this is that changes in the ethical values of a society can change its science in subtle but important ways. We might also draw another moral: the fact that a particular set of values shapes scientific practice at any particular time is not necessarily any vindication of that set of values. Nazi science, for example, might well rely on Nazi values. Claims regarding lethal quantities of food additives might be differently treated where Aryans and non-Aryans are at stake much as we treat such claims differently where humans and chimpanzees are at stake. If some sets of values can be twisted or perverse, some forms of science—those forms of science that rely on such values—can be equally perverse.

The background of values that Rudner points out in science is a background that may shift. In that sense the background values that have so far been indicated for our science are what I will term *nonessential* values; science would still be science were these values to be replaced by others.

Are there also *essential* values within science; values such that any "science" that abandoned or replaced those values would no longer be science?[9] Here I want to consider first some values that might be proposed as essential to science, in order to reject them. I then hope to propose some central values that I think *are* genuinely essential to science.

In the preceding piece, Edward Conze portrays science as a naked manifestation of a mere will to power. He blasts "science-bound philosophers" for their "ruthless will for boundless power," and says of science that "all that it does is to increase 'man's' power to control his 'material environment,' and that is something the yogic method never even attempted." Conze concedes to science its success in this regard, but "to judge all human techniques by the amount of bare 'control' or 'power' they produce is patently unfair."

But must science be committed to a lust for this type of power, or is it always so committed? Does Conze's "ruthless will for boundless power" indicate a value *essential* to science? Certainly not. Science can be pursued,

and often has been pursued, for its own sake and for the sake of mere knowledge. Paleontology and archaeology are dubious pursuits for any who lust for power, and not every form of scientific endeavor promises some high-powered technological application. It must be conceded that science *has* often been pursued with technological goals, and it is quite likely that our science would not have the shape it does today were that not the case. But it is also the case that religion has often been used for petty political ends. It no more follows that power is essential to science than that petty politics is essential to religion.

Consider also a second feature that is occasionally suggested as essential to science. It is sometimes proposed that science is in some way committed to, or is an elaborate apologetic for, an "everyday common-sense world of sense experience."[10] In this regard Conze speaks scathingly of the "narrow provincialism" of "sense-bound consciousness."

Is science essentially committed to "the everyday common-sense world of sense experience"? I am not sure what power this would have as a condemnation of science even if true. But in fact nothing could be much farther from the truth. Anyone who delves into contemporary physics expecting to find a familiar universe neatly expansive in absolute space and punctually sequential in time, filled with comfortably solid middle-size objects, will be very much surprised. From the perspective of common sense and everyday experience the claims of contemporary physics will seem among the most bizarre imaginable. The contemporary physicist seems as willing as any yogi to reject "the everyday common-sense world of sense experience" as illusion, and seems as insistent as any yogi that the more fundamental reality lies beyond it.

Neither a lust for power nor a commitment to an "everyday common-sense world of sense experience," then, appears to be essential to science. But there are, I think, values that *are* essential to science; values without which no pursuit could properly be considered science.

The first of these essential values involves an almost obsessive emphasis on truth. The aim of science is not to ennoble or to edify, to satisfy or sanctify or save. The aim of science is simply to sort claims on the basis of their truth or falsity. Within science no other criterion for sorting claims—on the basis, say, of accordance with religious tradition or political appeal—is even a close second. This is not, of course, to say that no claim has ever been accepted within a scientific community for baser motives than a sincere search for truth. But it is to say that any adoption of claims for other motives is to that extent *un*scientific, and that a "science" that abandoned this central emphasis on truth would no longer be science.[11]

Tied to this first essential characteristic is a second: science *demands* demonstration.[12] If a claim is to be adopted on truly scientific grounds, it

must be openly *demonstrable*, and no other appeal—to venerated authority or to the dicta of the properly initiated—will do.

This emphasis on demonstration has sometimes been ridiculed as a myopic devotion to phenomena that fit neatly within the confines of a laboratory. That, I think, is a mistake; science, though essentially committed to demonstration, is committed to no particular type of demonstration. Field studies, expeditions, and the appearances of comets have played a major role in the history of science. Contemporary reliance on mathematics reflects a willingness to accept a priori deductive as well as inductive demonstration. And there are times when the course of science quite properly shifts on the basis of what appear to be almost purely philosophical arguments.[13]

It does not appear, then, that science is confined to any particular type of demonstration. Nevertheless commitment to demonstration, of one type or another, seems essential; a "science" that abandoned or even significantly weakened the demand for demonstration would no longer be recognizable as science.

Here, as with regard to the emphasis on truth, we must concede that the demand for demonstration is an ideal of scientific practice, not always fulfilled. Science has at times been swayed by misplaced authority and has settled too quickly for claims without demanding sufficient demonstration. But that an obsession with truth and a demand for demonstration are *ideals* of scientific practice does not mean that they can be waved cynically aside. That these *are* the ideals of scientific practice, even if "merely" ideals, is crucial; anything that did not value these as ideals would no longer be science.

It appears, then, that science is "value-laden" in two importantly different ways. As noted in the previous section, standard scientific judgments of "compelling evidence," "sufficiently high probability," and "adequate establishment" demand a background of values. These values, I have argued, can shift and hence are not *essential* values; though *some* set of background values is required by science in this regard, no *particular* set of values is demanded.

But there are also particular values within science that are essential. A purported lust for technological power and a supposed slavery to an "everyday common-sense world of sense experience" are not among these. But an obsession with truth and a commitment to demonstration are; each of these is a value essential to science in the sense that a "science" without them would not be science at all.

Science and Its Rivals

It appears that we must reject, then, the first image of science and values presented above. Science is far from value-free; it is both shaped by a general

background of values and essentially committed to some particular values.

But at this point it is tempting to replace the first image of science and values with a second. It is quite often claimed, at least within some circles, that there are equally worthy alternatives to science. The general claim is that there are bodies of knowledge or types of pursuit that in some way conflict with science but that have an equal or greater claim to our serious attention. Conze appears to hold that forms of Buddhism are the equal or the superior of science, Alan Watts makes similar claims for similar religious traditions, Colin Wilson and Philip Slater hold that the occult somehow embodies a respectable rival to science, and Carlos Castaneda portrays the "world" of a fictitious Yaqui sorcerer as an equally acceptable alternative to the "world" of science.[14]

In the general spirit of this general claim, however, quite different particular claims have been made by different authors, or even by the same author at different times.[15] A complete treatment of all such claims would involve carefully disentangling and clarifying each claim, seeing what support can be offered in its behalf and what can be said against it. That complete task is well beyond us here. Instead I shall concentrate on only a sampling of such claims; those that rely on the "value-ladenness" of science.

If science is "value-laden," as we have argued that it is, is it not then on all fours with any other value system? Does not devotion to science then amount to dogmatic adherence to but one value system among many? Is not then the choice of those values allied with science as ultimately arbitrary as the choice of any alternative set of values?

These are, of course, rhetorical questions; those who pose them expect us to answer "yes" and to concede that science can claim no special status over rival values; that the adoption of scientific values is on a par with the adoption of those values evident in Christianity or Buddhism or Hinduism, or for that matter in Nazism or the fantasies of Carlos Castaneda.

The force of such an attack should not be underestimated. In this regard it may help to use a distinction hinted at above: a distinction between the current practice of what we think of as our scientific community on the one hand, and what we might call *truly* scientific practice on the other. The actual practice of those who staff our laboratories and conduct our various research programs may not always be fully or genuinely scientific. As you read this someone somewhere may be fudging a result, or misrecording an observation so as to support a pet theory, or doctoring the data. There may also be claims applauded by our current scientific community that are not genuinely scientific claims, backed by truly scientific evidence and subjected to genuinely scientific scrutiny. Current practice has its share (although I doubt that it has more than its share) of frauds and foibles and human frailties. And current practice may be *un*scientific to a greater or lesser degree because of such things.

But those who press the type of attack at issue are not merely criticizing "science," in the sense of current practice, for the extent to which it is *un*scientific. We would not need them for that, and they are out for bigger game. To the extent that current practice is *un*scientific, we hardly need an appeal to "rivals" or even a concentrated examination of values to condemn it. And to the extent that claims applauded by the scientific community are not genuinely scientific claims, we hardly need an appeal to "rivals" or a concentrated examination of values to condemn them. *Un*scientific aspects of current practice and claims currently accepted without genuinely scientific warrant are clearly to be condemned on purely scientific grounds, and would be whether or not science had any "rivals" in the sense at issue.

Those who press such an attack are instead questioning current practice precisely to the extent that it *is* scientific. Were it *perfectly* scientific they would still urge Buddhism and the occult and Yaqui sorcery as equivalent rivals or alternatives. Thus the full force of the attack is against science itself, in no matter how pure a form, rather than merely against the variously corrupted forms of current practice.

How good is the attack? *Does* it follow from the fact that science is "value-laden" that it is on all fours with any rival set of values?

As noted, science is "value-laden" in two importantly different ways: it is shaped by a background of inessential values, and it is also committed to some particular values. Consider first, then, those values that function as background to our science, and that shape our notions of "compelling evidence," "sufficiently high probability," and the like. To these values—the relative values we place on human and animal lives, for instance—there are clearly alternatives. Instead of *these* values we might adopt others, and if some religious tradition emphasizes importantly different values, we might come to adopt those values instead. We might decide that all life is as precious as human life, or that Brahmin life is of greater value than the life of lower castes.

But are these rivals to the background values of our science really rivals to *science*? I think not. As we have seen, science demands *some* background of values of the sort Rudner indicates. But it does not demand any *particular* set of values. A form of science shaped in this way by other values than those that shape our science would nevertheless be a form of *science*. So the choice here is not between science and those other values; it is merely a choice between a science shaped by the values by which our science is shaped and a science shaped by other values. If we are to set up a genuine conflict between science and its supposed rivals, then, it must be a conflict regarding something other than merely the background of non-essential scientific values.

Consider then the essential values we have claimed for science, which would seem much more likely candidates for a genuine clash between scientific and other values. If science is *essentially* committed to certain

values, is it not *then* on all fours with its alternatives? Have we not *then* shown that the choice between science and its rivals is simply a choice of values?

In order to answer this question carefully we must pay closer attention to the different *types* of things that may be at issue as "rivals" or "alternatives" to science. The term "science" is itself used ambiguously enough that what is meant is sometimes the *activity* of science and sometimes a set of *claims* accepted on scientific grounds. Hence science is sometimes contrasted with other activities or pursuits, and in considering rivals or alternatives we are considering rival or alternative activities or pursuits. But science is also sometimes contrasted with rival sets of claims—the claims of science versus the claims of Genesis, for example.

Let us take these one at a time. Does science, with its essential values, clash with other activities? Not, I think, in the sense that objectors to science commonly have in mind. Essential to the activity of science is an obsession with truth and a commitment to demonstration. And clearly this is not the case with all activities; art, music, and poetry, as well as certain forms of asceticism and meditation, are quite different. Does that mean that one must pursue science, devote oneself to truth and demonstration, and abandon these other activities? Not at all. There are other ways to live one's life, and in some respects—or for some people—these other ways may be infinitely preferable. But that there are other activities that are rivals to science in *this* sense is surely no condemnation of science, any more than one's choice to pursue a career in business is any condemnation of poetry (or vice versa). If this is what objectors to science have meant by portraying science as on all fours with other endeavors, they are certainly right; there is nothing in science or its essential values that absolutely compels us to do science or to emphasize those values. But it would seem odd to characterize this as in any significant respect a *challenge* to science.

So far, then, we have not found a genuine clash between science and its supposed rivals, and thus have not found the clash between science and "equally worthy rivals" suggested by its critics. Alternatives to the inessential background values of science are not alternatives to *science*, because science could assume any of various sets of background values. And alternatives to the activity of science, despite its essential values, are merely other worthy pursuits rather than "rivals" in any more threatening sense.

Finally, however, science is often set in contrast to other sets of claims. And here, unlike in the previous two cases, we do seem to have a genuine conflict. It must be remembered that we are dealing with *genuinely* scientific claims, and not all and only claims accepted by those who staff our laboratories may qualify as genuinely scientific. But even so, there are undoubtedly some claims current within some religious traditions, and some claims accepted within some cultural groups, that will simply contradict genuinely scientific claims. On pain of contradiction, one cannot accept both.

Does it follow from this, together with the fact that science is essentially "value-laden" in the ways specified, that the claims of science are on no firmer ground than any of a number of rival sets of claims, or that each is equally worthy or unworthy of our attention? Certainly not. The mere fact that two sets of claims conflict in no way forces us to throw up our hands, or to suspend judgment, or to pick a set at random. There may be much that can be said for one set rather than another. And the particular values essential to science, far from relegating it to the level of any of various alternatives, may offer a significant argument in its behalf.

At least the following can be said for the claims of science. Science is essentially committed to truth and demonstration, and genuinely scientific claims will reflect those values. They will have been selected with an obsessive ideal of truth and through the patient toil of dogged demonstration. This does not, of course, guarantee their truth; genuinely scientific claims may nevertheless be false and claims adopted without scientific warrant may nevertheless be true. But it does mean that scientific claims will be backed by a wealth of demonstration, evidence, and argument. Only further demonstration, better evidence, and stronger argument can rationally force us to abandon them; and then, of course, we will be replacing some scientific claims with other scientific claims. None of this can be said for most sets of claims that appear as religious or cultural tradition.

The essential values of science, then, are scientific *virtues*, and as genuine virtues are not to be flippantly waved aside. Science *is* essentially committed to particular values, but the values at issue are ideals of truth and demonstration. Those are ideals that are not to be taken lightly and are not to be lightly put aside.

On what basis, then, might one choose to abandon scientific claims and to adopt some other set of claims instead? One might do so by abandoning the essential values of science, or by relegating them to a secondary position. One might take something else as more important than truth, and choose claims contradictory to those of science on the basis of some other value; aesthetic or (broadly) political or personal appeal, perhaps. Or one might take something to have precedence over demonstration; the words of sacred authority or of sacred texts, perhaps, or the counsels of some form of mystical experience. But here other claims will have been accepted on the basis of *other* values, not on the basis merely of truth and demonstration. One will be able to appeal for one's chosen claims on the basis of whatever values they embody. But if they are to be chosen on grounds other than merely ideals of truth and demonstration, one cannot expect to be able to defend them solely with an eye to truth and solely on the basis of demonstration.[16]

One might also, of course, choose to adopt some set of claims—however unpopular—and to demonstrate their truth. This is a very different case. If science can adopt any form of demonstration, as argued above, one will not

then have abandoned science. One will be defending one's claims as rival scientific claims, rather than as rivals to scientific claims, and will be contributing to the scientific project rather than renouncing it. One may, indeed, be contesting the accepted claims of current practice. But that is how science proceeds.

None of the arguments considered above, then, shows that science has equal rivals in the sense sometimes proposed by its opponents. Science *does* rely on a background of values, and there are rival values to those on which our current science relies. But this does not mean that those rival values are rivals to science itself. Science *is* one activity among many, and others may for various reasons be rightly pursued instead of science. But these other activities are not *rivals* in the threatening sense that critics of science generally have in mind. Finally, scientific claims will be contradicted by other sets of claims. But it does not follow that these need be *equal* rivals. Genuinely scientific claims, *because* of the essential values of science, have ideals of truth and demonstration on their side.

We have, then, found reason to question each of the images of science and values with which we began. Science is not value-free, and is moreover essentially committed to some particular values. At least with an eye to the arguments considered, however, it need not follow that science, because "value-laden," is on all fours with any of various "rivals" or "alternatives." The essential values of science, far from indicating a fatal weakness, may be its greatest strength.

Notes

1. It is perhaps appropriate to mention also two general topics that I will not attempt to deal with in their entirety. The first is value-relativism. Implicit in both images of science and values above is an assumption that all values corrupt, or that any introduction of values must represent capricious dogmatism, or that all "systems" of value are equally worthy—and hence equally unworthy—of our allegiance. I do not think any of these relativistic assumptions is true, but I will not here be able to argue fully against any of them. Second, it must be said that arguments based on the premise that science is value-laden are only some among the many arguments, not usually distinguished, that attempt to show science to be no better than various rivals. I do not think any of the other arguments are any better than those explicitly addressed here, but in what follows I will concentrate only on arguments that in one way or another turn on the issue of values.

Some of the claims of this essay—particularly those in the final section—are posed in a slightly stronger form than I might choose in another context. For present purposes, and because my intent is to offer a corrective to exaggerated views on the other side, I think this is relatively harmless.

Conze and James are represented by the two preceding papers. Carlos Castaneda's views are scattered throughout *The Teachings of Don Juan: A Yaqui Way of Knowledge* (Berkeley: University of California Press, 1968), *A Separate Reality: Further Conversations with Don Juan* (New York: Simon & Schuster, 1971), *Journey to Ixtlan: The Lessons of Don Juan* (New York: Simon & Schuster, 1972), *Tales of*

Power (New York: Simon & Schuster, 1974), and *The Second Ring of Power* (New York: Simon & Schuster, 1977). See also Richard De Mille, *Castaneda's Journey: The Power and the Allegory* (Santa Barbara, Calif.: Capra Press, 1976), and Richard De Mille, ed., *The Don Juan Papers: Further Castaneda Controversies* (Santa Barbara, Calif.: Ross-Erikson, 1980).

2. That (6) is perhaps controversial is of no importance to the argument here; "Contemporary handling of nuclear wastes is perfectly safe" or "It has not yet been clearly determined whether or not contemporary handling of nuclear wastes is unsafe" would serve our purposes equally well. The same applies to (8).

3. It has become quite widely accepted that the notion of health is value-laden. See Joel Feinberg, *Doing and Deserving* (Princeton: Princeton University Press, 1970), pp. 253–255; and H. Tristram Engelhardt, Jr., "Human Well-Being and Medicine: Some Basic Value-Judgments in the Biomedical Sciences," in *Science, Ethics, and Medicine*, ed. H. Tristram Engelhardt and Daniel Callahan (Hastings, N.Y.: Institute of Society, Ethics, and the Life Sciences, 1976). Both of these are reprinted in Thomas A. Mappes and Jane S. Zembaty, *Biomedical Ethics* (New York: McGraw-Hill, 1981).

4. Richard Rudner, "The Scientist *qua* Scientist Makes Value Judgments," *Philosophy of Science* 20 (1953): 1–6; reprinted in Baruch Brody, ed., *Readings in the Philosophy of Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1970).

5. For the sake of simplicity I have not dealt here with a complex argument to the effect that "the 'softness' of social facts may affect the 'hard' notions of truth and reference" (p. 46), which appears in Hilary Putnam's *Meaning and the Moral Sciences* (Boston: Routledge & Kegan Paul, 1978).

6. A science that scrupulously avoided values in this way, of course, would be a science that was itself of little value.

7. This view appears in Richard C. Jeffrey, "Valuation and Acceptance of Scientific Hypotheses," *Philosophy of Science* 23 (1956): 237–246; reprinted in Brody. But Jeffrey concedes, in the spirit of the following paragraph, that "this account bears no resemblance to our ordinary conception of science."

8. To choose to value human and chimpanzee lives equally, or Rembrandts and belt buckles equally, would of course not be to abandon a background of values but to choose a particular background. As Rudner notes, science could attempt to escape *any* background of values only by abandoning notions of "adequate establishment," "sufficient evidence," and the like.

9. This is all too brief as an introduction of essential properties, let alone of essential values. I hope that it will nevertheless prove adequate for the task at hand.

10. This phrase appears in scare quotes because I am not sure precisely what a "world" is here supposed to include, nor am I sure—even when most sympathetic—that there is only one such "world."

11. This claim does not, I think, conflict with Rudner's conclusion, appealed to above. The fact that various values may be crucial to what we accept as "adequate evidence" for the truth of a claim does not mean that we are ultimately concerned with anything less than the *truth* of the claim.

It might also be noted that even claims that have slipped into currency by illegitimate means may later be offered proper scientific support. I am indebted to David Pomerantz for discussion on this point.

12. The tie between an emphasis on truth and a demand for demonstration is as subtle as it is intimate. The more sincerely one is concerned with truth, the more emphatically one will demand demonstration. And the more one demands demonstration, the better our reason to believe that one is committed to truth. Both of these

claims hold only ceteris paribus. But the ceteris-paribus status of each need not indicate that the tie between truth and demonstration is therefore a weak one.

13. Many of the arguments for behaviorism of some forms, against evolutionary "emergence" in biology, for important claims regarding quantum mechanics, and regarding tachyons traveling backward in time seem to be fully "philosophical" arguments.

14. Conze is represented by the preceding piece, and Castaneda's work is mentioned in footnote 1. For other authors mentioned, see Colin Wilson, *The Occult* (New York: Random House, 1971), quoted in the general introduction; Philip Slater, *The Wayward Gate* (Boston: Beacon Press, 1977); and Alan Watts, especially *Psychotherapy East and West* (New York: Vintage Books, 1975).

15. Here Conze might serve as a convenient example. At one point Conze criticizes science for ignoring the accounts of yogis: "There is ultimately only one way open to those who do not believe the accounts of the yogins. They will have to repeat the experiment . . . they will have to do what the yogins say should be done and see what happens" (p. 300). If this is an attack on science at all, it is a pretty weak attack. What Conze calls for here is *more* science—a scientific investigation of the claims of yogis—rather than for some alternative to science. Yet a few pages later Conze claims that the world of the yogi is one incapable of scientific exploration; within the magical world "the phenomenon vanishes when the full light is turned on" (p. 304), and within the spiritual world "it is quite impossible to ever establish any fact beyond the possibility of doubt" (p. 304). Here Conze changes his tune and presses a quite different attack: "The methods of science, mighty and effective though they may be, are useless for the exploration of two-thirds of the universe, and the psychic and spiritual worlds are quite beyond them" (p. 304). This is a quite different claim from that proposed earlier, and calls for quite different argumentative support. Can it be *demonstrated* that "two-thirds of the universe" is beyond the reach of science, and that claims made by Conze or anyone else about this major portion of the universe are true? If claims about this portion *can* be demonstrated, and if—as argued in the previous section—science can adopt any form of demonstration, then these claims are within the potential grasp of science after all. If these claims *cannot* be demonstrated, of course, we lose at least one good ground for believing that they are true at all, or for believing Conze's claim that we have overlooked a major portion of the universe.

16. Here it might be claimed that yogic meditation, for example, involves its own unique form of demonstration. I do not think that is true; what is at issue seems to me entirely unlike anything we normally consider "demonstration." But if there *is* a legitimate form of demonstration buried here, then, as argued above, it is grist for the mills of science. What remains to be demonstrated (and this is by no means a unique matter in the history of science) is that there *is* a form of demonstration at issue.

Suggested Readings

As noted in the general introduction, this is not intended as anything like a complete bibliography. The works mentioned are generally standard sources, for the most part easily available, which may be of help at the next stage of the reader's research.

The attack on science evident in Edward Conze's "Tacit Assumptions" appears in various forms in the following:

Castaneda, Carlos. *The Teachings of Don Juan: A Yaqui Way of Knowledge* (Berkeley: University of California Press, 1968).

Pearce, Joseph C. *The Crack in the Cosmic Egg*. New York: Pocket Books, 1973.

Pirsig, Robert M. *Zen and the Art of Motorcycle Maintenance*. New York: Bantam Books, 1976.

Slater, Philip. *The Wayward Gate*. Boston: Beacon Press, 1977.

Watts, Alan. *Psychotherapy East and West*. New York: Vintage Books, 1975.

Wilson, Colin. *The Occult: A History*. New York: Random House, 1971.

Critical works:

Evans, Christopher. *Cults of Unreason*. New York: Dell, 1973.

De Mille, Richard, ed. *The Don Juan Papers: Further Castaneda Controversies*. Santa Barbara, Calif.: Ross-Erikson, 1980.

Gardner, Martin. *Science: Good, Bad, and Bogus*. Buffalo, N.Y.: Prometheus, 1981.

Sagan, Carl. "Night Walkers and Mystery Mongers: Sense and Nonsense at the Edge of Science." In *Broca's Brain*. New York: Random House, 1979.

Sladek, John. *The New Apocrypha: A Guide to Strange Science and Occult Beliefs*. New York: Stein & Day, 1973.

General journal sources include *The Humanist*, which is overwhelmingly critical in tone, and *Zetetic Scholar*, published by Marcello Truzzi of the Department of Sociology, Eastern Michigan University, Ypsilante, MI 48197, which attempts to be a less biased forum.

On science and values, see especially:

The section on "The Acceptance of Scientific Theories," in Brody, Baruch, ed. *Readings in the Philosophy of Science*. Englewood Cliffs, N.J.: Prentice-Hall, 1970, pp. 539–570.

Martin, Michael. "Value Judgements and the Acceptance of Hypotheses in Science and Science Education." *Philosophical Exchange* 1 (1973): 83–100.

Putnam, Hilary. *Meaning and the Moral Sciences*. Boston: Routledge & Kegan Paul, 1978.

On ethical and epistemological relativism:

Brandt, Richard. "Ethical Relativism." in *The Encyclopedia of Philosophy*, ed. Paul Edwards. New York: Macmillan and the Free Press, 1967.

Gardiner, Patrick. "Irrationalism." In Edwards, *Encyclopedia of Philosophy*.

Ladd, John, ed. *Ethical Relativism*. Belmont, Calif.: Wadsworth, 1973.

Meiland, Jack, and Krausz, Michael. eds. *Relativism: Cognitive and Moral*. Notre Dame: University of Notre Dame Press, 1982.

Montague, Phillip. "Are There Objective and Absolute Moral Standards?" in *Reason and Responsibility*, ed. Joel Feinberg. Fifth edition. Belmont, California: Wadsworth, 1981, pp. 480–491.

Stace, Walter T. "Ethical Relativism." Reprinted from *The Concept of Morals*. New York: Macmillan, 1937. In Feinberg, pp. 492–497.

Wilson, Bryan, ed. *Rationality*. Evanston, Ill.: Harper & Row, 1970.

Index

Philosophy of Science and the Occult

Patrick Grim, Editor

Philosophy of Science and the Occult has two aims: to introduce the philosophy of science through an examination of the occult, and to examine the occult rigorously enough to raise central issues in philosophy of science.

Patrick Grim has compiled selections by authors with divergent views on astrology, parapsychology, and UFO's to emphasize topics standard to the philosophy of science. He discusses issues such as confirmation and selection for testing, possibility and a priori probabilities, causality and time, explanation and the nature of scientific laws, the status of theoretical entities, the problem of demarcation, theory and observation, and science and values. A sketch of where these arise in the collection accompanies the table of contents. The context of the occult serves to make the initial introduction of these issues immediately understandable and forcefully compelling.

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